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ORIGINAL LECTURES.

THE FUTURE OF THERAPEUTICS.

The Annual Address to the Detroit Medical and Library Association.[1]

BY DAVID INGLIS, M.D.,

THE RETIRING PRESIDENT.

GENTLEMEN:—Another year in the history of our Association brings us to-night to a point of observation from which we naturally look back to note progress made, and forwards to plan how best to fulfil our coming duties and opportunities. In such a retrospect perhaps one of the most striking facts, and one of which our Secretary's interesting statistics make no note, is that during the entire year of active work in the Society there has occurred no instance of animosity or contention between members, but rather we have harmoniously worked together.

Therein, I conceive, lies a valuable lesson as to the uses and value of a medical society. We are proud to count a membership of ninety-five active members, but prouder to show ninety-five physicians who have put aside personal animosities and united in a common effort for a common good. The experience of such a society also demonstrates that much of the ill-feeling which arises among medical men can be obliterated if they will but come in contact with one another. There is a treasure of philosophy in the statement that "no man is as good as his best deed nor as bad as his worst." Our professional life is, in its nature, one of an unusually personal character, and we are, all of us, in danger of taking too personal a view of events. We are too much in danger of judging some professional brother by his worst act. It has happened, not once but repeatedly, in this Association, that meeting as we do, in a more general relation, some of us have learned that others are by no means as bad as our worst act had seemed to indicate.

On the other hand, such a society just as surely tends to bring out the fact that we are, none of us, fully as good as our best deed. This process of friction, while sometimes disagreeable to the subject, is uncommonly salutary, and tends to a higher average of good work. In a word, the year's work has brought about an increase of good feeling and promoted many friendships. It would be a great gain, to many localities in the State, if working medical societies were formed and existing societies vitalized into more generous life.

As part of the retrospect of the past medical year it might be stated, that an amendment in the organization of the State Medical Society has been proposed by which that Society shall be changed into a delegate body. It would seem to be a long step in advance as tending to compel the formation of local societies and to raise the value of membership in the State Society. The great need of our profession is unity—no other profession allows personal animosity to so weaken its power as does our own.

So, we would hold up our Association as a living example, yet modestly withal, for there are o thers in the same station. Indeed, to-night it is our great pleasure to welcome, as we do, so many members of our local sister societies. It is a pleasant testimony to the growth of friendly feeling that we can thus come together. While we each pursue our work in the coming year, let us carry ourselves simply as regiments in the same brigade, each jealous of our own honor, but all engaged in one purpose.

To the gentlemen from neighboring cities, as well as the unattached of our own, we give a cordial welcome, and are honored by their presence.

In reviewing the past year there is one fact which can well be mentioned with pride, and that is, the number of pathological specimens which have been presented; enough, indeed, to have furnished working material for a good pathological society. Such an amount of material indicates increasing thoroughness of scientific investigation on the part of our members, and ought to serve as a reminder that we can, with much advantage, during the coming year, devote more time to pathological discussion. It also recalls the fact that a large part of the reported cases met with in the journals are shorn of half their value by the statement "no *post-mortem* was allowed:" nothing confirms a diagnosis like a post-mortem examination. Sometimes it disproves the diagnosis.

In the year's work of the Society, one element has done much to maintain the interest and promote vigorous life, I refer to the plan of having set debates appointed. This plan seems to be unusual in medical societies, but its working has proved most successful; there is an unmistakable freedom of expression and criticism, and much is gained by the vigorous discussion of both sides of a question.

In a retrospect of the year's work and contemplating it, as we do, with pleasure, I cannot forbear

[1] Delivered on October 4, 1885.

embarrassing our Secretary by a testimony to his unfailing zeal and attributing to him, in greatest measure, our success. And our retrospect brings to our minds that which the years unfailingly bring, the gathering of the ripened grain. We are all conscious that our work and our efforts are but part of the necessary growth of life, and "the end crowns the work." We ought to look forward pleasantly to the time of its completion, and, indeed, it is with pleasure that we recall how nobility of character in each, crowned lives which, in their short space, were of benefit and pleasure to us and to all whom they influenced. Yet the fields look barren in the autumn, and we could wish that Reynolds's cheery voice and good judgment and Pease's hearty good-will to every body were with us yet.

There are many other things which such a retrospect suggests, but it is to the future I would call your attention. What are the duties and opportunities which lie before us? To such a question there arise many answers, but from among them I would dwell upon one:

WHAT IS TO BE THE FUTURE OF THERAPEUTICS?

It is obvious that much of the scientific work of the day is done by men who entertain but slight regard for faith in therapeutics, who themselves seem not to possess such a faith, and whose writings and statements of facts are such as to foster skepticism in regard to therapeutic possibilities. It can hardly be denied that, as a result of this, there prevails, in certain quarters, a more or less pronounced skepticism, and that among practicing physicians. Now, the blame for this state of things does not lie wholly at the door of those pathologists who can conceive of no disease which does not have its origin in some demonstrable lesion, some atrophy or malformation, and who pass over functional diseases with contempt or an apology; but this lack of faith originates, fully as much, from the one-sidedness and the inefficient methods of therapeutical investigation itself. It is a good deal here as in religious matters; many people will not join the church on account of the claims of the world, the flesh, and the devil, but about as many more keep out because of the inconsistencies of those who already are church members. Now, it behooves us who have faith in therapeutics to mend our ways. And, to my mind, the first thing to be done is to more accurately define and establish the *place of action* of our various drugs. While it is obviously true that, the body being a whole, general bodily states in many cases give the indication for treatment, yet it is also true that, not only in diseases distinctly local, but also in general systemic diseases, our therapeutical endeavors are directed to localized functions. Instances could be given without number, but the mere mention of the fact suffices. In anæmia, rheumatism, scrofula, the various fevers, and, indeed, in about all the general diseases, the constant demand upon the physician is to modify the function of one or other organ. Herein, I conceive, lies the success of the skilful physician; of many physicians using the same general group of drugs, one will achieve better results than the rest simply by a more accurate judgment of the adaptation of the remedy.

This brings us to the consideration of the next thing which ought to be done, namely: we should more thoroughly work out the detailed symptoms of each case. It is much easier to formulate in our mind certain gross symptoms and, coupling them with certain physical signs, make a diagnosis of a *disease*, than it is to bear in mind, beside the grosser symptoms, the lesser manifestations, the finer shades, and to make the diagnosis thus: *A patient in a certain condition.* If we hold in our minds the idea of a pneumonia, for instance, we are not far from falling into some routine treatment of pneumonia; but if we have in mind the patient with a process going on which not only, tangibly enough, interferes with the function of respiration, but, just as certainly, with the function of the nervous centres, the digestive, glandular and circulatory organs, and that the danger may make its appearance at any point, we will not only find indications for prognosis in the varying lesser symptoms, but still more important indications for treatment. In other words, we have fallen lamentably behind in the study of symptomatology.

The most cogent claims which medicine makes to be considered as one of the sciences are based upon the accumulated facts of pathological and physiological research. The pathologist, in spite of his therapeutic skepticism has just cause for pride; so, too, has the surgeon. Time was when the surgeon-barber was held in low esteem, but our surgical brethren not only claim for themselves, but are credited by the laity with the highest place. Their claim is based like that of the pathologist, upon accumulated and unquestionable facts.

Now it would seem that the only permanent basis for a scientific Therapeusis is to be found in an exact knowledge of two kinds of facts: *first*, what is the precise effect of a drug upon the functions of healthy organs; and *secondly*, what is the effect upon organs whose functions are already disordered?

The first class of facts are being accumulated in a gratifying manner by a number of competent investigators; the second class must, in the nature of things, be accumulated by the practitioners in direct contact with the sick. Such men as Dr. H. C. Wood, in investigating the first class of facts, pursue a very definite course. The drug to be studied is used *alone*, of definite strength, accurate dosage; phenomena observed are recorded with accuracy and verified by many repetitions. Suppose that we apply that plan to our purposes as practising therapeutists. Is it not evident that it would work a revolution in the methods of practice, and in the place of therapeutics in medical science! It would first do away with complicated prescriptions; it would compel the physician to pay attention to the quality of the drug used, he would ascertain how long it had been kept in stock, the reliability of the manufacturer and the accuracy of the dispenser. He would be compelled to note accurately, not in his fallible memory, but in records which could be referred to again, all the variations both in the symptoms existing when the drug was first administered and the modifications of the symptoms *produced by the drug*. I do not say after the

drug was given, but "produced by the drug," for by this plan we would soon reach that certainty which constitutes a *fact*.

It may be replied that all these things are being done already, and I grant that here and there are men who are following out these lines, and who can justly claim to be scientific physicians; but a perusal of the insufficient notes taken by those whose notes become public in connection with reported cases, and a single thought of those who never take notes at all, establishes the fact that general practitioners have not yet fairly taken in hand the problems which general practitioners only can solve. On the contrary, there is abundant and painful proof that the profession as a whole is seriously retrograding. We have just said that the scientific physician is one who accurately applies his remedies to accurately observed conditions to produce constant results; what shall we say of physicians who prescribe patent medicines!

I do not refer to "Dr. Pierce's pleasant purgative pellets," or "Ayer's sarsaparilla," or "Dr. Hartley's great South American cure." We all agree in our condemnation of these and their kind, their makers and their makers' methods. There may be some virtue in them, probably is; but not knowing their component parts, we cannot tell of their proper uses. They are vaunted to cure such a multiplicity of diseases that we at once deny the claim. They are advertised in what we consider an obnoxious manner; we will none of them! I hold in my hand some interesting medical literature. The first specimen is got up in somewhat startling red type, and is a treatise upon nervous diseases. Celerina, I find therein, is of inestimable value in over fifty different diseases. It would take too long to mention them all, but a host of letters from the afflicted prove, among other things, that, to say nothing of spermatorrhœa, impotency and allied nervous diseases (?) locomotor ataxia, malignant jaundice and mollities ossium gracefully yield to this compound, which we are told contains in each fluid drachm the equivalent of "five grains each of celery, coca, kola, viburnum and aromatics." The neurologist's occupation is practically gone.

Here I hold a volume upon "Aletris Cordial." I do not know what is in aletris cordial, but seventeen diseases acknowledge its sway, and with seventeen gynecological diseases (among which, curiously, is typhoid fever) counted out the gynecologist is left but a stubble-field. We are not *quite* sure what constitutes "Fellows' Syrup of Hypophosphites," but a beautifully bound series of text-books upon "Nervous Diseases," "Pulmonary Diseases," etc., informs us of its remarkable effects in forty-six different diseases.

These are great in themselves, but they pale their ineffectual fires before "Dr. Hayden's Viburnum Compound." A similar obscurity hangs over the contents of this prescription, and words fail me in giving any adequate account of its virtues. Caulocorea, Iodia, Horsford's Acid Phosphates, Listerine, Tongaline, Dyspepsine, Quineptus, Morrholine, Nephrilene, Asthmaticine, these and many more keep our medical journals alive in more ways than one. I do not need to go further; the plain and pitiful fact stares us in the face from nine-tenths of the medical journals, that the medical profession has allowed itself to become debauched; it has abrogated its function, and it is impossible to draw any line between the nostrums which the people buy and those which many physicians prescribe. I add Ayer's and Hostetter's almanacs to the pile; they are, on the whole, rather more modest than some of the others.

No, gentlemen, it is high time that we enter our protest and that we, each of us, purge ourselves. The present methods are unjust to the people, for they are entitled to scientific therapeutics at our hands, and to just prices for their medicines. (That they pay an unjust price, is evident from the fact that one firm engaged in the manufacture of one of these articles, is said to have paid last year, a dividend of 75 per cent. on its capital stock.) These methods are unjust to our pharmacists, for they cannot conduct legitimate pharmacy at a fair profit. These methods are unjust to ourselves, for we are lowering the standard of our professional life.

The future of therapeutics will be this: The profession will find that using and recommending private and semi-private formulæ, as well as the vast array of elixirs of unknown strength and composition, will result in serious detriment to all parties concerned, and self-preservation will compel recourse to scientific therapeutics.

Let us all begin now.

ORIGINAL ARTICLES.

THE PHYSIOLOGICAL ACTION OF THE DIFFERENTIAL PNEUMATIC PROCESS ON THE CIRCULATION.[1]

BY E. TIEGEL, M.D.,
OF NEW YORK.

A series of physiological investigations, the major part of which has been made only of late years, can so easily be brought into relation with the manner in which Dr. Williams' process reacts on the body that one might easily think that the invention of this apparatus was a consequence of these investigations. But on the contrary, the current of thought was a completely independent and original one. For this very reason it, is the more useful and necessary to show that in the pages of theoretical medicine this apparatus, which for a considerable time has stood a number of practical and clinical tests with brilliant results, can be freely proved and its action physiologically deduced. To accomplish this, we have to refer to the researches of a series of authors, among whom we shall mention Einbrodt, Quinke, Pfeiffer, Zuntz, Knoll, and Yager.

A number of necessary experiments have also been made with the pneumatic cabinet in which I have been assisted by my colleague and associate, Dr. W.

[1] See the following articles: Dr. Herbert F. Williams, N. Y. Medical Record, January 17, 1885; Editorial in the JOURNAL OF THE AMERICAN MEDICAL ASSOCIATION, February 14, 1885; Dr. V. Y. Bowditch, Boston Medical and Surgical Journal, July 16, 1885, and JOURNAL OF AMERICAN MEDICAL ASSOCIATION, August 1, 1885; Dr. A. S. Houghton, JOURNAL OF THE AMERICAN MEDICAL ASSOCIATION, November 7, 1885; Dr. P. C. Jensen, same JOURNAL and date; Dr. W. Everett Smith, same JOURNAL November 14, 1885.

A. de Watteville, of New York, and Mr. Joseph Ketchum, of Brooklyn.

Before proceeding further, let me present a short description of the instrument and the process:

The pneumatic cabinet as at present constructed is of sufficient size to accommodate a patient seated within it. In communication with it is a bellows, or cylinder, of such size that when opened to its full capacity it withdraws sufficient air from the interior of the cabinet to produce rarefaction to the extent of, say, one-thirtieth of an atmosphere. This done, the patient is instructed to place his mouth over the orifice of a flexible breathing tube, and the air from the outside is allowed to flow into his lungs in its effort to replace that which has been withdrawn. The extent of this inflow is the amount of his respiratory capacity, plus the pressure of, say, one-thirtieth of an atmosphere. Expiration is thereupon effected by the muscular effort required to overcome the increased atmospheric pressure. The amount of air respired by this means, and at this degree of rarefaction, is increased by from 20 to 100 per cent., depending largely, of course, on the individual respiratory capacity.

In the following discussion I speak only of the process just described, leaving to other investigators to determine the therapeutic scope of the instrument as an aid to respiration by the reverse of the above, and by the ability of the operator to effect complete artificial respiration by inspiratory and expiratory aid, or force synchronous with the normal respiration of the subject.

Let us examine what would happen in our body if the heart stopped, and yet the respiration proceeded normally. If such a condition could be produced, and life could co-exist, we would have without the respiration exactly the same manometrical pressure wherever we measured it. In the advent of an inspiration the pressure in the cavities of the pluerae would necessarily increase, the lungs be more aspirated than before, the capillaries more expanded, their volume increased, and a certain quantity of blood from outside the thorax aspirated into it. In this case we would get an inspiratory wave in the vascular system. Let us call any movement of air or blood, the direction of which is *towards* the thorax, an *inspiratory movement*, and any movement *away* from the thorax an *expiratory movement*, or wave. This inspiratory wave may show itself in the veins only, and not in the arteries of the body, because the semilunar valves of the aorta prevent any other movement than the one towards the capillaries of the body. Therefore we have an inspiratory depth wave in the venous system. In case of a forced inspiration the pressure in the cavities of the pleuræ is 40 mm. less than in the atmosphere, and therefore the venous wave would be large.

At the end of an ordinary expiration the pressure in the cavity of the pleuræ is only 6 millimeters less than the pressure of the external air. During such expiration the pressure therefore increases 34 mm., and the volume of the lung capillaries must correspondingly decrease, and we have now an expiratory wave, which, according to the mechanism of the valves of the heart, must be felt principally in the the arterial system of the body. With a forced expiration we can bring the pressure on the alveolar side of the lungs to more than 30 mm. mercury above the atmospheric pressure, and thus compress the blood vessels in the thoracic cavity, which naturally must cause a greater positive expiratory wave than the ordinary passive expiration. We would thus be able to sustain a sort of circulation by forced respiration even in case the heart stopped. This circulation would have the same direction as the real one, produced by the heart. Physiologists have for some time cherished the belief that the functions of the heart were not affected by the relations just explained, and that the action of *respiration* or *circulation* would simply be added to the action of the heart. This is the reason why they thought that the arterial pressure was greater in expiration than in inspiration. But this was fundamentally wrong. The first sphygmographical experiments proved that besides undulations of arterial pressure, caused by the pulsation, there exists another system of undulations much slower in movement and with much longer waves. It was proved to be isochronous with the respiration, and therefore was supposed to be caused by it. It is a system of waves upon which the pulsations are *superimposed*. Without closer examination they declared that the ascending portion of the respiratory wave coincided with and was caused by the expiration, and that the descending portion stood in the same relation to inspiration. Einbrodt was the first who registered upon one and the same paper, at the same time, both the respiration and the blood-pressure, and thus proved that the ascending branch of the respiratory wave of the blood-pressure corresponds to the inspiration and the descending branch to the expiration. But both systems, that of the respiration and that of the respiratory movement of blood-pressure, are temporarily displaced towards each other.

In ordinary respiration the blood-pressure ascends during inspiration and sinks during expiration. But the highest point of each undulation of blood-pressure is immediately after the beginning of expiration, and its lowest point immediately after the beginning of inspiration. Such is also the case in deep respiration, where the expiration lasts longer than the inspiration. It is not noticeable that the succession of pulsations are quicker during inspiration than during expiration. If the respiration be very quick and shallow, we cannot find a decided influence of the respiration upon circulation. If the breathing is deep and slow, if, for instance, the pneumogastric nerves are irritated, we have the same undulations of pressure as in the first case, but their frequency is not affected.

In explaining these facts we must keep in view that undoubtedly the expiratory diminution of the thoracic volume is favorable to the transportation of blood out of the thorax, and that the inspiratory enlargement must cause an aspiration of blood; but it is equally sure that these factors are more than compensated by others equally important. These must be now examined, and we shall find them to consist in the influence exerted by the respiratory acts upon

the celerity with which the blood runs from the right heart through the lungs to the left heart.

If we remove the lungs from the thorax and compare the amount of blood which passes through them, both in equal time and under equal pressure, we find that this amount varies according to the condition of the lungs, viz.: if the latter are collapsed, the amount of blood is greater; if, on the other hand, they have been insufflated from the trachea, the amount will be smaller. In the living body, however, the lungs are never expanded by means of such insufflation, but always by the inspiratory movement of the thorax, by means of which the pressure is lowered on the pleural side, and so the air is sucked into the lungs through the trachea. But if we intend to imitate this artificially on excised lungs (for which, of course, we need special apparatus), we find that, under otherwise equal conditions, more blood will pass through the lung which has sucked up its air in the manner described.

The net of capillaries in the alveoli is so arranged that by the expansion of the lungs their transverse diameters are increased, and therefore the resistance to the flow of blood is diminished. This is the reason why the blood will pass rapidly through a lung which has filled its air cells by suction. When, however, the lungs are insufflated, they compress the capillaries to the extent of increasing the resistance to the flow of blood. We understand now why, during inspiration, more blood passes from the right to the left heart than during expiration. In addition, we have during inspiration a lowered pressure in the whole pleural and mediastinal space, and hence over the heart and great vessels inclosed in the thorax. This lowered pressure, however, makes itself hardly felt on the left heart with its thick muscular walls, and on the rigid coats of the aorta, but the opposite is the case with the large veins and the right heart, which suck up blood in consequence.

In *inspiration*, therefore, the right heart has more blood, and drives it more easily through the lungs. A greater quantity of blood will pass into the left heart and aorta, and for this reason the blood pressure will rise in the arterial circulation.

There are two ways in which the heart alone can augment the average arterial pressure. One is if the pulsations follow each other more quickly, the other is when the amount of blood which is driven into the aorta is increased in each individual pulsation. Both ways may coincide under certain circumstances.

One phenomenon still remains to be explained. It is the retardation which the tops of the respiratory blood pressure waves show in comparison with the tops of the immediate respiratory curves. But this apparent exception is indeed the best demonstration of the correctness of our explanation. We said: The volume of the pulmonary vessels is greater during inspiration than during expiration; hence there must be a change of volume at the beginning of every act of respiration. In other words, we shall have a diminution at the beginning of expiration and an increase at the beginning of inspiration. The blood which in the former case is displaced from the lungs must flow into the left heart and into the aorta. Therefore we have the highest pressure at the commencement of expiration, but immediately afterwards the pressure sinks. When, at the beginning of an inspiration, the volume of the pulmonary vessels is augmented, the increase must be filled out with blood, and for a short moment, that is, for one or two pulsations, the left heart gets less blood than before, and the pressure in the aorta sinks to its minimum.

Waldenburg, as is well known, devised an apparatus in which it is possible to make a man inhale either compressed or diluted air. He was of the opinion that the arterial pressure is increased by the inhalation of compressed air. We know now that the experiments which brought him to this opinion do not justify his conclusions. He applied a sphygmograph to the radial artery of a man and made him inhale compressed air. He now found that the average position of the lever of the sphygmograph was higher than before. But this phenomenon is not caused, as Waldenburg believed, by an increase of arterial pressure; it is caused, on the contrary, by an *increase* of the *venous* and a decrease of *arterial pressure*. The blood is stemmed in the capillaries and veins, in consequence of which the tissues are swelled, especially those lying beneath the sphygmograph. Experiments upon animals prove the correctness of this explanation. The animals were prepared in such a manner that the blood pressure could be measured immediately with a manometer. The most interesting ones for our purpose are those made by Zuntz. He used the apparatus of Waldenburg, which was connected with the trachea of a dog in which the arterial pressure was registered with the kymograph. The possible variations were using, (a), compressed or diluted air in, (b), inspiration and expiration. The results are the following:

1. Expiration *into* compressed air, arterial pressure *sinks*.

2. Inspiration *of* diluted air, arterial pressure *rises*.

The respiratory undulations were higher in both these cases than ordinarily.

3. Inspiration *of* compressed air, arterial pressure *sinks*.

4. Expiration *into* diluted air, arterial pressure *rises*.

The respiratory undulations were lower in both cases than in ordinary respiration.

It is now not necessary to give a detailed explanation why these experiments prove the correctness of our opinion on the influence of respiration upon circulation. If, in case 3, the respiration was continued for a long time, the blood pressure finally rose and its respiratory undulations were strongly expressed. But this is caused, very probably, by dyspnœa and contraction of the small arteries of the body caused by it. In addition to this apparatus of Waldenburg, many other forms of respiratory apparatus based on the same principle have been applied to the human respiratory organs. How far they could have reacted on circulation can easily be deduced from the principles we have explained in the foregoing pages, so that further details are unnecessary.

Let us now reflect how to arrive at the solution of the following problem: *How can we increase the*

arterial blood pressure in the body by means of respiration? To arrive at this result we must not blow up the lungs, but expand them by suction. In other words, we must enlarge the chest. To enlarge the chest without muscular force we must diminish the air pressure which compresses it. We see that such a manner of reasoning would guide us directly to Dr. Williams's differential process. We have simple means to prove the correctness of our opinion regarding the essential physiological effects of the apparatus upon the human body. It will be interesting to show that the quantity of blood driven out of the chest by the left ventricle is greater when a person is inside the cabinet in reduced atmosphere, and breathes normal air, than if he is outside breathing the normal air. In the latter case, his body and chest walls are submitted to the same air pressure as the air he uses for respiration. At the first glance we are tempted to employ a sphygmograph. But the error committed by Waldenburg warns us not to use this instrument.

Let us suppose the pericardium, with the heart included, was completely rigid and immovable, and there was between its walls and the heart no space at all, or only an incompressible fluid. If now the systole of the left ventricle should squeeze out of the heart 180 ccm. of blood (about its normal capacity), it can only do this if, in the same time, 180 ccm. venous blood return to the heart. But pericardium and thorax are in reality elastic, not rigid, and the 180 ccm. contained in the left ventricle immediately before the contraction, leave the thorax in a fraction of the time of a whole pulsation. But the venous blood which flows from the body into the heart has the time of one whole pulsation to do so. There must, therefore, at the end of every systole exist aspirating forces which influence (a) the venous blood, (b) the air of the lungs, (c) the walls of the thorax, diaphragm, and abdomen. It is only b in which we are interested. Here we can demonstrate an inspiratory movement of the air, caused by every systole, by joining the trachea of an animal with a Marey's tambour (or Marey's diaphragm), and by arresting the respiration for a short time. But also on man this negative pulsation can easily be demonstrated by joining the cavity either of the nose or of the mouth with a manometer containing a few drops of colored water. We now close all openings (mouth and nose), arrest the breathing, and open the glottis. On doing so we notice a leaping or bobbing of the water synchronous with the pulsations. The first half of the wave is an inspiratory one, that is, the first and quick part of the pendular movement is towards the chest. The second part of the movement is constantly interrupted with dicrotism, In case the glottis is closed (few men are able to govern their glottis in such a manner as to keep it open when told to stop breathing) the drop makes again an excursion synchronous with the pulsation, but it is in an opposite direction. The drop is moved outwards because now the wave of the pulsation fills the arteries of the buccal and nasal cavities during its passage. Hence the volume of these cavities is slightly diminished. We understand now that the movement which we see when the glottis is open is a difference between two antagonistic movements, and that the systolic aspiration of air is in reality more considerable than is indicated by the manometer.

In repeated trials we have compared measures to make sure whether the excursions of the drop are greater after an inspiration inside the cabinet or not. Without exception we obtained the same result, and we found that they are greater during the sojourn in the cabinet, and are therefore an excellent proof that the quantity of blood thrown out of the left heart into the aorta by one pulsation is greater during respiration inside the cabinet than outside. To further prove that the arterial pressure is raised, we must show that the number of pulsations is not essentially decreased by the apparatus. If a man, whether healthy or sick, be put for the first time into the cabinet, and if he experiences for the first time the dilating influence upon his chest, his pulsations show some irregularity, which disappears as he becomes acquainted with the manner of breathing.

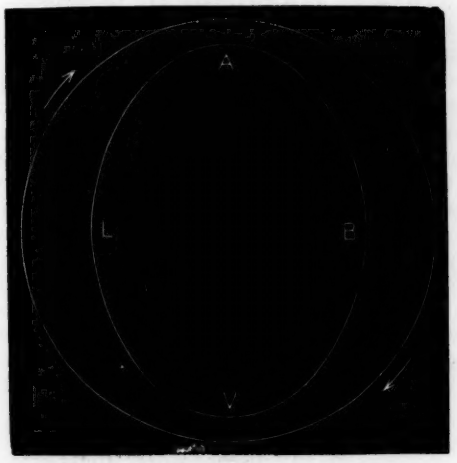
In every case the psychical and physical accommodation is gained after some time, and then we have the general rule that the main number of pulsations taken in one whole minute is greater inside the cabinet than outside, During the inspiratory state of the chest the frequency of pulsation is greater than during the expiratory state, the difference inside the cabinet being much greater than would be the case normally. The following is the outcome of an experiment made which gave, on frequent repetition, the identical results:

Normal breathing outside the cabinet, pulse.....	70
Inside the cabinet, with diminution of seven-tenths inches, average mercurial pressure.	
On light (not labored) respiration.....	88
On full inspiration held.....	104
On full expiration held (after allowing pulse to quiet down).....	60
Ordinary pulse after breathing in the cabinet through the tube and then shutting the stop-cock.....	80

That is arranged

Normal outside.....	70
Normal inside.....	80
During respiration inside.....	88
On full inspiration.....	104
On full expiration.....	60

The changes produced in the pressure and rapidity of blood current by the use of the differential cabinet can be nicely illustrated in the following manner: Suppose we have a walking path or ring (as shown in figure). It is narrow at the two points A and V, and wide at the points B and L. Suppose now a great number of pedestrians have a cause of interest at heart to walk as fast as possible around this path in the same direction as indicated by the arrows. In this case the following conditions obtain: At the two places where the pathway is narrow the crowding will be great. At the places where the way is wide the crowding will be little. Let us call now the broad part, L, the lung-capillaries, and the broad part B, the body capillaries. The pedestrians are the blood corpuscles, and the cause of interest which keeps them moving is the heart's pressure. We now intend to widen the portion of the



pathway, L, through the lung-capillaries (as is done in reality by the differential cabinet). This will produce the following changes: 1. The time in which the crowd, otherwise the blood corpuscles, is able to go round the whole circuit once, is shorter, because the road to be traveled over is at one place wider and easier. The rapidity of the blood-current is therefore increased. 2. From V (vena cava) to A (aorta) the crowding—otherwise the blood-pressure—is decreased, because the pathway is wider. 3, In the part extending from A to V the crowding (or blood-pressure) is increased because the crowd (otherwise the blood) gets quicker from V to A than it would do under normal circumstances.

[*Note.*—The comparison which we have used in this instance is only correct when, in every part of the pathway, both before and after the widening of the path mentioned above, the length dimensions (mathematically considered) are very great in proportion to the breadth of the path. If the latter be increased to a considerable amount, then the result of this widening is a lengthening of the road which every blood-corpuscle has to travel, therefore a retardation and not an acceleration of the circulation, and besides a lowering of the blood-pressure. This may be the case in our own circulation, especially when we have got an extreme dilatation of the large vascular system of the abdomen.]

In regard to the question whether the pressure of the respired air is the same in the alveola as in the trachea, we observed occasionally that the water drop in the heart manometer was drawn violently towards the lungs of the experimenter, when, with a view of watching the drop pulsations during arrested respiration, we opened too soon the stop-cock which shuts off the lungs from the heart-manometer during respiration. The air in the alveoli had not yet arrived at the normal atmospheric pressure. But if the experimenter rested for only a few seconds in the inspiratory position, and then began the experiment, the drop always undulated up and down in a certain equilibrium. As a pressure of less than one-tenth of a millimetre of mercury would displace or expel the drop, this fact is the surest evidence that even in the farthest alveolus the air-pressure can be brought to be the same as the atmosphere, if we rest only a few seconds in the inspiratory position of the chest.

Very interesting and important observations made on patients can be explained by this fact. I mean the arrest of pulmonary hæmorrhages by the application of the cabinet. Indeed, if the pressure in the lung-capillaries and its small vessels sinks, those are the very parts from whence these hæmorrhages tend to come, and if the air-pressure remains the same, or is even increased, it is perfectly plain how the hæmorrhage can be stopped.

According to observations made, the succession of pulsations is quicker during inspiration than during expiration, providing the way of breathing is such that any difference can be observed. We made some experiments with the view of determining if inside the cabinet the same relations exist. The experimenter remained for five seconds in inspiration, and and then for five seconds in expiration, and counted his pulse in both cases. In inspiration the frequency of the pulse was considerably greater than in expiration. All professional men who have breathed inside the cabinet agree that the thorax scarcely ever reaches the normal expiratory position, but on the contrary the inspiration surpasses by far the ordinary state, and *if performed slow enough* it reaches the highest standard possible. *Respiration is now performed not with the respiratory air only, but with the complementary plus the respiratory air.* The inhaled and the expired volume of air is considerably greater than in ordinary respiration. Moreover, there is the following important fact: In ordinary respiration, inspiration is the active and expiration the passive act, but inside the cabinet we have the reverse. *Inspiration is passive and expiration active.*

To show objectively that the type of respiration is changed, we made the following experiments: If one breathes through a large tube to which a mercury manometer is attached in such a manner that the lateral pressure of the inspiratory air-current can be measured, we fail to observe any movement in the mercury as long as the tube is wide enough. But the slightest obstacle, as a finger, crossing the opening without closing it, is sufficient to induce the mercury to move, and then we see that the inspiratory excursion of the mercury is greater than the expiratory one. In applying the same disposition to our experiment the experimenter entered the cabinet breathing the normal air through the tube. The mercury showed only an expiratory movement, because now no active aspiration is needed to bring air into the lungs.

In close connection with the question we have discussed thus far, there are others of not much less importance. They regard the exchange of gases inside and outside the cabinet. It cannot be doubted that only special experiments can give a complete certainty in this respect. But we know certain physiological facts, which can be demonstrated, to show what, in all likelihood, will be the state of things. The greater fractions of both the essential gases contained in the blood, that is of O and of CO_2, are held there in a loose chemical combination. The nature of such a combination causes that the quantity of

both gases, contained in a given quantity of blood, can change considerably, but that their tension changes only slightly. The tension of O especially is so low that mammals and man can absorb O enough from an atmosphere wherein the tension of this gas is only 50 mm. mercury. Any given quantity of blood can be saturated with O at this tension, but the process goes on much slower than it would do at a higher tension. The higher the tension is the quicker the blood is saturated with O. We know that during one respiration the heart makes four pulsations. In ordinary circumstances 4 + 180 = 720 ccm. of blood pass through the lungs during this space of time, and are transformed from the venous into the arterial state.

If we should do the same thing outside the body by shaking 720 ccm. venous blood with air, we would take much longer time. The reason for this is that inside the lungs the blood is spread over an extended surface where it is influenced by air. If we fulfil this condition outside the body by making the blood flow along the walls of large glass tubes, through which an air-current runs, the arterialization is performed in a similarly short time. There are two factors which favor the oxygenation of blood: Firstly, *the increase of contact surface between air and blood;* and secondly, *the increase of the partial pressure or of the tension of O in the air.* These factors can replace each other. Observations have been made on the population of the Highlands of South America and on other mountaineers. Many of them suffer from a peculiar kind of anæmia, which can be cured by sending them to live in low altitudes, best of all at the sea shore. From others, otherwise strong and healthy, it is reported that their chest and lung capacity is larger than usual. If the blood has to remain a little longer in the lungs, and if it is spread over a greater surface, it absorbs the same quantity of O during one respiration that it does at a higher pressure but less surface.

Between the limits of human habitation the variations of the contact surface are much more important for the gas exchange in our body than any geographical or meteorological variations of the air-pressure. It is very likely that a person suffering from a reduction of the respiratory surface, whatever disease it may be caused by, absorbs more oxygen inside the pneumatic cabinet. The remarkable effects of the pneumatic process as a general and special tonic, which clinical experience has shown, has been demonstrated in the able and interesting article of Dr. H. F. Williams, in the *Medical Record*, of Jan. 17th, 1885. For a healthy man the former statement cannot be made, because the increase in the velocity of the blood movement through the lungs will compensate the possible influence of the increased respiratory surface. But such an increase would in most cases be useless by itself. The arterial blood of a healthy person is entirely saturated with O, and the influence of the apparatus can be a mechanical one only. But in case of a person suffering from a reduction of respiratory surface, we have a different state of matters. Physiology teaches us that in dyspnœa caused by the insufficient ventilation of the blood, the arterial pressure increases. It is increased not because the quantity of blood contained in the arteries of the body is greater than in the normal (this quantity is, on the contrary, less), but it is increased by a simultaneous contraction of the arterial walls. This phenomenon is caused by nerve influence, especially by the accumulated carbonic acid in the blood running through the medulla oblongata and spinal cord. If now, in consequence of the influence of the pneumatic cabinet, we get an increase of the contact surface between blood and air, the blood will be better ventilated, and by degrees the cramp-like contraction of the arterial walls will disappear, so that in such a case the apparent effect is contrary to the one produced on normal bodies. It will depend on certain circumstances if this decrease will be followed by an increase corresponding to the normal one.

The effect just explained, however, can but prove highly useful in any disease where the blood wants more O, and where it is overcharged with CO_2.

We finally come to the question concerning the exhalation of carbonic acid. This process is, within large limits, independent of the absorption of O as far as mere quantities are concerned. But we see that a condition which favors the one, generally also favors the other; and therefore we may suppose that more CO_2 is discharged inside the cabinet than outside, the difference being the greater, the greater the difference of the absorbed quantities of O.

In regard to the use of the apparatus in cardiac therapeutics, the physiological conclusion of our argument must be evident, Its use is indicated in every case in which we have a deficient oxygenation of blood, and where the state of the cardiac and arterial walls do not render a sudden dilation dangerous, as, for example, would be the case in fatty or sclerotic degeneration.

In closing, I have to render my thanks to my friend and associate, Dr. W. A. de Watteville, of this city, to whose able assistance both in conducting the necessary experiments and in the preparation of the text itself, I am much indebted.

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EXOPHTHALMIC GOITRE, OR GRAVES' DISEASE.

BY J. W. THOMPSON, M.D.,
OF ST. PAUL, MINN.

During the past ten years that I have been engaged in the practice of ophthalmic medicine and surgery, I have had the opportunity of seeing a considerable number of cases of exophthalmic goitre. The pathogeny and treatment of this remarkable disease are very unsettled and therefore very unsatisfactory. With a view of adding something to my limited knowledge of this subject beyond what I was able to gather from its literature, I adopted, in the beginning, the practice of keeping somewhat extended notes of every case of the disease that came under my observation. I have selected five typical cases for the subject of this paper, and herewith present the results of my observations to the readers of the JOURNAL.

Palpitation of the heart, enlargement of the thyroid gland, and prominence of the eyeballs formed a marked trio of symptoms which were present in all five of the cases selected. Each one of these great symptoms occurred in point of time in the order above mentioned. The length of time between the occurrence of these symptoms in their regular order was not constant. The longer the interval between the heart disturbance and the enlargement of the thyroid gland, the more unyielding was the malady to remedial agents and the more protracted and intense was the suffering of the patient. One of the five cases occurred in a male 30 years of age. Of the four females the disease occurred in one before the menopause, and in the other three at or about that period. The heart palpitation in the male was very distressing several weeks before the other symptoms made their appearance. In his case the enlargement of the thyroid was very slight, while the protrusion of the eyeballs was very considerable. In all other respects he was healthy and robust and had always enjoyed good health. There seemed to be nothing in his individual history that furnished any apparent cause or foundation for the disease. Upon investigating his family history, he stated that one member of the family, a brother, had in early youth suffered with "fits" as he termed it. A careful inquiry revealed the fact that the "fits" had undoubtedly been epilepsy. Possibly this peculiar neurosis manifested itself in the one by epilepsy and in the other by exophthalmic goitre.

Of the four cases in females, in three of them, the three characteristic symptoms exophthalmos, goitre and quickening of the heart's action were well marked. In the fourth one the enlargement of the thyroid was confined to the left lobe. Over the right lobe only a very trifling fulness could be detected. The prominence of the eyeballs was very great. At times the insertion of the recti muscles could be seen. The eyes became very sensitive to the light, and in one a central corneal ulcer formed, which is a complication said by some authors never to occur. The severity of the symptoms was so intense that the patient was confined to her bed the greater part of the time for a period of nearly four months. Under appropriate treatment the ulcer healed kindly, the patient made a good recovery, and four years afterwards was enjoying good health. It is over a year since I lost sight of her, and I am unable to say whether there has been any recurrence of the disease.

The general symptoms in all the cases, except the male, were very like. Great excitability, anæmia and emaciation were present in a very marked degree. The rapid and forcible action of the heart produced a rapid, jerky, trip-hammer pulse. The carotids throbbed perceptibly and the pulsation of the abdominal aorta could be very distinctly felt. In one of the cases I found the pulse as frequent as 170 per minute. In none of the cases did I find it below 100. There existed in all an extremely intensified self-consciousness. The most trivial exciting cause produced flushing of the face, great increase of the heart's action, and occasionally much dyspnœa, which in one case, became at times rather alarming. In this case however, some of the dyspnœa could be readily attributed to the excessive enlargement of the thyroid. The enlarged thyroid increased the size of the neck to such a degree, that the collar she formerly wore was too small by four and one-half inches.

Among the general symptoms, besides the palpitation of the heart, goitre and exophthalmos, there were a number of minor symptoms which were more or less present in all the cases, and proved at times annoying and even distressing. These were indigestion, hepatic disorder, pains in the extremities, local dropsy, sleeplessness, headache, vertigo, ringing in the ears, nausea, and vomiting after meals, diarrhœa, hæmorrhage from the bowels, nose-bleed, night-sweats, and in two of the cases hæmoptysis. There was also much irritability of temper. The mind became at times very capricious and suspicious, feeble and childish.

One of the prevailing theories of the day is that this peculiar disease has its origin in the nerves of organic life. From the observations that I have had the opportunity of making, I have become a little skeptical on this point. It seems to have been established more by assertion than by proof. With a view of throwing some light, if possible, on the correctness or incorrectness of this theory, I made a very careful record of the temperature at stated times in each and every case that came under my observation. In each instance I found it above the normal. It ranged from 99 to 102. It was usually increased when, from any exciting cause, the action of the heart was accelerated. The temperature was always taken on each side of the body at the same time, and it always registered the same; *e. g.* the correspond-

ing cheeks, the sides of the neck and the axilla. The normal difference between the cheeks and the axilla was observed in each instance, with one or two slight exceptions hardly worthy of mention. It has been quite conclusively shown by experiment that the ganglionic chains have an action entirely independent of each other. If the cervical portion of the ganglionic system be involved, as is claimed, then there would necessarily exist a difference of temperature between the corresponding sides, since it is impossible for both chains to be affected equally in the outset and progress of every case of this disease.

Another observation made in relation to the five cases, under consideration that opposes the sympathetic theory, was the prompt response of the iris to the action of light and shade, and to atropine. The writer does not claim that the phenomena furnished by five typical cases alone could possibly establish a correct theory of the origin of a disease, which furnishes such a multitude of symptoms having such a wide range of deviation. He does claim, however, that the sympathetic theory is quite inadequate to explain all the phenomena observed in these cases.

Dr. Hammond says in his celebrated work on "Diseases of the Nervous System" (page 819, seventh edition): "I am inclined to think that in the present state of our knowledge we are scarcely warranted in locating exophthalmic goitre in the sympathetic nervous system, and that we are justified in regarding it as an affection of the brain and medulla oblongata."

The link that connects this disease to ophthalmology is the local effect it produces upon the eyeballs. This is, however, only one of its pathognomonic symptoms. Yet, it is a symptom that produces a hideous deformity and has given much notoriety to the disease. It gives to the eyes a wild, staring expression. They project forwards from the orbits in many instances, so far that the lids cannot close over them entirely. The prominence of the globe became so marked in one case that the lids at no time, not even while sleeping, completely covered the cornea. A very interesting case of this nature is given in Trosseau's "Clinical Medicine," vol. 2, page 178. He says: "Dr. Pain kindly supplied me with further details of this interesting case. Twice in the course of a year there came on such a paroxysmal increase of the exophthalmos, that one of the eyeballs became dislocated as it were." "The eyelids got behind the circumference of the eyeball, which had to be pushed back with a certain amount of force in order to get the lids to come forwards again." This prominence of the eyeballs is due largely to hyperæmia of the orbital cushions, and in the chronic cases, there is undoubtedly some hypertrophy. Müller's muscle, which has the power of projecting the eye forwards in health, is supposed by some, to act as one of the projecting factors in this deformity. In these extreme cases of prominence, the cornea is constantly exposed to the irritating action of the atmosphere and dust. Occasionally, in very chronic cases, the conjunctiva of the upper lids becomes much thickened and the surface of the cornea uneven and vascular.

In most of the cases that I have been able to observe carefully, the prominence of both eyes was about equal, and as nearly as I could ascertain occurred simultaneously. In many of the cases, however, the extent of the protrusion was rather deceptive. The eyelids seemed in some instances to become retracted towards the orbit and favored, as it were their lodgment behind the equator of the eyeball. It requires but a small increase in the width of the sclerotic zone to give to the eye the appearance of more prominence than in reality exists. In one case I observed a considerable degree of anæsthesia of the cornea, so that the finger could be passed over it gently without producing but slight discomfort to the patient. I believe this is the exception and not the rule. In the cases of extreme prominence, I found hypermetropia in a much higher degree than I had reason to believe existed before the occurrence of the exophthalmos. This was the mechanical result produced by the enlargement of the orbital cushions, which pressed the globe forwards against the resisting lids, and thus resulted a shortening of the autero-posterior axis of the globe.

The prognosis in this disease is usually favorable, though when required to express an opinion with reference to any particular case, it is prudent to be guarded, since cases have been known to terminate fatally. I never saw but one case having a fatal termination. In certain mountainous regions in France, this disease has been known to prevail somewhat as an endemic, and occasionally one has had a fatal termination. When death does occur, it is usually the result of general exhaustion and a gradual failure of all the vital forces. The victim is by degrees worn out.

The treatment is about as unsettled as the etiology. The materia medica has been pretty thoroughly ransacked without avail for a specific. At one time and another I have employed a variety of treatment. Iodine exhibited externally and internally seemed to be attended with more favorable results as far as my experience has gone than any other medicinal agent. Yet, in some of the cases, the hygenic and diatetic regulations, together with a change of climate when the circumstances of the patient permitted, had undoubtedly a favorable influence in bringing about a favorable result.

I think the only safe plan of treatment is to endeavor to guide the malady to a favorable end. To attempt to cut it short by specific measures, seems too much like striking in the dark. Many times I believe it is difficult to say whether the patient is cured or got well.

St. Paul, Oct. 25, 1885

ABSCESS OF THE LIVER.[1]

BY J. W. HAYES, M.D.,
OF MARIANA, ARK.

I do not propose to enter into an exhaustive discussion of the subject of this paper. Nor do I deem it at all necessary, at this time, to deal too minutely with a subject so generally understood by all physicians and surgeons as abscess of the liver. But in justification, somewhat, for the present detail I will

[1]Read before the State Medical Society of Arkansas, April 22, 1885.

say this, that the inexperienced upon this subject will find very little to comfort or console them by searching our standard authorities. They all state that this is a disease that prevails in Egypt, in the East and West Indies; and one author states that it is occasionally seen in the boatmen of the Mississippi River.

That it does exist, and exists in sufficient abundance, all up and down our Southern water-courses and other places where miasma abounds, is a truth that cannot be questioned, and urges every practitioner to ever be on the alert for hepatic abscesses in all cases of chronic disturbances of this organ.

When we have a well-defined case of abscess of the liver pointing outwardly, we have a case easy of diagnosis, and easily treated; the prognosis of which, with proper treatment, I would always deem favorable. But when the abscess is deeply seated, or pointing internally, the diagnosis is especially difficult, the operative treatment tedious and dangerous, and the percentage of deaths far greater. To distinguish these latter from encysted, or fatty tumors, from hydatids, and, in fact, to distinguish them at all, the aid of the exploring needle must frequently be called into requisition. An intelligent history of the case, however, will reveal enough, with the few physical symptoms that are developed, to almost make certain our diagnosis.

But just here we might ask: What use have we for the diagnosis? When many of our standard authors say that we must not operate on the deep-seated abscesses at all; that they are sure to die; at the same time confessing that the fluid, left to itself, without an operation, will some day break internally, and by so doing create a certain death.

Bégin, of France, recommends the following operation for deep-seated abscesses of the liver: Make an incision some two or three inches long upon the most prominent part of the swelling, and divide with great caution the layers of the abdominal wall, until the peritoneal linings are reached; raise this carefully with the forceps, nick it and afterwards divide it. Upon reaching the surface of the tumor, the operation must be temporarily arrested, the wound filled with lint and dressed; the patient to remain three or four days for adhesions to take place, after which we open into the abscess with reasonable hopes of success. Several years ago, the Medico-Surgical Society of Alexandria published the statistics of 123 cases, of which only forty-two were operated upon, and eighty-one without an operation. Of the whole number seventy-nine died; in nine the results were not ascertained. It is of interest to note that of those not operated upon eighty per cent. died, and of those who were operated upon only fifty per cent. died. It seems to me, with a limited experience and knowledge of the disease, that the death-rate here cited in both classes of cases is higher than it would be in our country.

A clinical case of much interest has recently come under my observation, a recital of which may be of interest to others.

Case.—Saw D. M., colored male, about 30 years of age, on March 2, 1885. He gave the following history: He had been a deck hand on a steamboat and a cotton picker up and down the Mississippi River for the past two years. Had been in bad health for six months; unable for two months to do anything. Was now complaining of gnawing pains in the right side, had frequent rigors, was somewhat dropsical, with enormous enlargement of the liver; jaundiced appearance of the eye, etc., making, in my opinion, a well-defined case of hepatic abscess. To these symptoms were added a dry tongue, tympanites, a temperatnre of 102°, and other symptoms of a slow and low grade of fever. A well-defined tumefaction presented itself in the centre of the hepatic region. Being a long distance from home, I determined to try the virtue of my lancet, though I was entirely alone with my patient. Without unnecessary delay I ran my abscess lancet one inch through the adjacent walls, and to my surprise and disappointment failed to reach an abscess. With an exploring needle I found pus and a cavity one inch and a quarter deeper. I now rode a half mile to procure a nurse and assistant to carry out directions. On returning, I made a free incision, whereupon a stream of sero-purulent fluid began to spurt out with considerable force. I pulled out an ordinary bucket, empty, from under his bed, and in twenty minutes it was filled with this fluid, the latter half of which was thick pus. All told, there could not have been less than eighteen pints of this fluid. I gave him no anæsthetic except a small dose of morphia. Left an alterative to be administered, to be followed by quinine.

On my arrival next day I found my patient worse. Temperature 104.5, some delirium, and symptoms pointing to septic infection. The alterative medicine previously administered had acted well, but the aperture in the side had closed up; no drainage had taken place for ten or twelve hours. On examination, I failed to find the peritoneal adhesions of the day before to the abdominal wall, and think that some effusion into the peritoneal cavity now took place. The adhesions doubtless were of recent date, and not firm enough to resist the pressure of fluid turned in this direction. I had previously bandaged the patient to aid drainage of the pus and to favor adhesions in the cavity, when drained. His face was badly swollen, and every feature was one of anxiety and distress. After carefully probing the wound to the liver, about three and one-half pints of pus were discharged. I saw him an hour later, when the swelling of the face had disappeared, as also did all his other unpleasant symptoms. I now left him, ordering tr. iron gtt. xv every four hours, alternating with turpentine emulsion—the latter for the fever symptoms, for its diuretic effect, and as a general diffusive stimulant. I left him this time confident that about twenty-four or forty-eight hours, at the outside, was his probationary period for this life.

I saw Mr. H. (on whose farm the patient lived), three days after my last visit, and was informed that the man was in fine spirits, had a good appetite, and was improving rapidly. I sent syrup of hypophosphites, to be taken in conjunction with the other treatment, and directed that the aperture be kept open, and a free drainage induced. He was left to nurses of his own race and color, who neglected this

important part of the treatment. The wound closed, the drainage stopped, and the patient died two days afterwards.

This is the fourth case of abscess of the liver that the writer has operated upon, and the only one of the four that died, though one of the other cases was a very delicate and nervous woman about 45 years of age, with bad sanitary and dietetic surroundings. The results of the operation in this case suggest the following conclusions:

1. Always operate if a reasonable diagnosis is made out.

2. Anæsthetics are not necessary in every case for this operation.

3. A free incision and a rational treatment warrant a more flattering prognosis, and a larger percentage of recoveries than our standard authorities lay down.

MEDICAL PROGRESS.

ANATOMY AND PHYSIOLOGY.

THE USE OF THE FOSSA AT THE LOWER END OF THE FIBULA.—MR. C. B. LOCKWOOD, in a note on this subject says: If the usual text-books be consulted, they will be found to say that the fossa, at the lower end of the fibula, gives attachment to the posterior fasciculus of the external lateral ligament of the ankle-joint. Examination of numerous specimens shows that this is only partially true, and that the most important use of the fossa has been over-looked. The ligament (posterior fasciculus of the external lateral) is attached into the lowest part of the fossa, quite close to the apex of the external malleolus. The upper part of the fossa serves for the reception of the ligament during dorsal extension of the foot.

By dorsal extension, is meant a straightening out of the foot upon the leg. Many anatomists call this action flexion of the foot, because the flexor muscles of the leg are concerned in its production.—*British Medical Journal*, Oct. 24, 1885.

MATERIA MEDICA AND THERAPEUTICS.

CASCARA SAGRADA.—This drug has been recently extensively studied in the service of M. DUJARDIN-BEAUMETZ, in the Hôpital Cochin. Its bark contains several resins, a peculiar crystallizable substance, a fixed and a volatile oil. These bodies have not yet received special study, and the drug is given in the form of the powdered bark or the fluid extract. Cascara sagrada is an excitant of the abdominal sympathetic, its special action being on the stomach and intestines; it is a purgative, and of considerable value in constipation. It has been used in America in intermittent and remittent fevers, but in France it is used solely for its purgative effect.

The powdered bark is given in doses of 25–75 centig., and sometimes in doses of 1 gramme. The fluid extract is given in doses of 0·50 to 1 gramme. The fluid extract is the best preparation. On account of its disagreeable taste, the following formula may be used:

Fluid extract of cascara	30 grm.
Simple syrup	30 "
Peppermint water	30 "

M.

Dose—3 or 4 teaspoonsful a day.

In cases of atomy of the stomach, with intestinal inaction or paresis, cascara may be combined with nux vomica.

Tincture of nux vomica	2 grm.
Fluid extract of cascara	20 "
Simple syrup	15 "
Cherry-laurel water	15 "
Distilled water	10 "

M.

Dose—3 or 4 teaspoonsful a day.

—*Nouveauz Remèdes*, Oct. 15, 1885.

MEDICINE.

INTRA-PARENCHYMATOUS INJECTIONS IN PNEUMONIA.—If, says LÉPINE, an injection of a few centimetres of a very weak aqueous solution of corrosive sublimate be made into the hepatised lung on the third or fourth day of the disease, in three or four places equidistant a few centimetres from one another, and preferably at the periphery of the lesion, with a view of preventing the extension of the disease, the following phenomena are observed: 1. At the seat of infection an immediate diminution of the crepitant râles and tubular breath sounds, which are in part replaced by respiratory silence and some larger râles; 2. sometimes, later, a transient exacerbation of the temperature of body; 3. the next day a great improvement in the general condition, and notably a precocious defervescence; and 4. a resolution which, to judge by the persistence of the "souffle," especially in the hepatised parts that have not been treated, takes place very much earlier than would have been the case under ordinary circumstances. As to the relative innocuousness of the intra-pulmonary injections in the doses employed (20 to 25 cubic centimetres of 1 in 40,000 solution of bichloride of mercury), when care is taken to keep away from the large vessels at the hilus of the lung, and not to penetrate the lung more than 3 to 4 cm. ($1\frac{1}{5}$ to $1\frac{3}{5}$ in.), M. Lépine urges that he has not lost a single patient and has not had one accident. The only inconvenience is the pain, but this is not great, and may be still further relieved by adding morphine to the solution. After the introduction of the sharp needle, and before the syringe is fitted on, a few drops of blood are allowed to escape; the injection must not be delayed or the needle will become plugged. When the needle is inserted into healthy lung or into tuberculous lung it does not as a rule yield blood. In the healthy lung such injections produce sufficiently defined lesions. Experiments on the lungs of healthy dogs showed that at the site of injection of a rather stronger solution than that mentioned above, there was a circumscribed and indurated area, which was made of blood and congestive œdema. The lesions were less marked with the 1 in 40,000 solution.—*Lancet*, Sept. 5, 1885.

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THE EDITOR OF THIS JOURNAL would be glad to receive any items of general interest in regard to local events, or matters that it is desirable to call to the attention of the profession. Letters written for publication or containing items of information should be accompanied by the writer's full name and address, although not necessarily to be published. All communications in regard to editorial work should be addressed to the Editor.

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THE NINTH INTERNATIONAL MEDICAL CONGRESS—WHY FURTHER OPPOSITION SHOULD CEASE.

All who have given due attention to the progress of the work of effecting the Preliminary Organization of the Ninth International Medical Congress will remember that the sudden and apparently concerted withdrawal of prominent members of the profession, in Philadelphia and two or three other cities, from further participation in such organization immediately after the first meeting of the Committee of Arrangements, which was held in Chicago in June last, was in its terms *conditional*. Each notice of withdrawal ended in substantially the same language, namely: "*as now proposed to be organized.*"

This language clearly implied two ideas: First, that there was something either in the Rules that had just been adopted by the Committee, or in the *personnel* of the organization, or in both, that those withdrawing could not sanction. And second, that if such objectionable features were removed, those withdrawing might renew their coöperation. The only Rule ever mentioned as objectionable was that relative to the conditions of membership in the Congress, which had been so changed by the Committee as to limit the American Membership to delegates from the American Medical Association and from such State and Local Medical Societies as were in affiliation therewith, thus cutting off delegates from all the National Organizations of Specialists. As soon as this proposed restriction of American Membership became known, it was declared unnecessary and injudicious by all parties. This, and the discovery that no provision had been made for an Executive Committee of the Congress to take charge of its interests after the Preliminary Organization had been completed, caused the Committee to defer any official announcement of the Rules or Organization until more mature consideration and another meeting had been held. But from the persistence with which the opposition denounced the Committee and created the erroneous impression in Europe as well as among their own friends at home, that the officers of the Committee were to be officers of the Congress, it was clearly apparent that much the larger part of the opposition was founded on purely personal considerations. We say the *erroneous* impression, because it must be kept in mind that the Committee as enlarged by the American Medical Association in New Orleans, at its first meeting in Chicago organized purely as a Committee of the Association to provide such necessary Preliminary Organization for the Congress as would enable it to assume full and independent charge of its own affairs. So far from claiming for itself or its Chairman and Secretary any official position in the Congress, the Committee adopted a rule at the commencement of its work, that it should appoint none of its own members to an official position in the Preliminary Organization of the Congress.

When the prejudices and passions of the hour have passed by, history will show that this much abused Committee of Arrangements was composed very largely of intelligent, conservative and truly representative men of the profession in their respective States; men who comprehended the nature of the duties assigned to them, and who were earnestly desirous of maintaining the harmony and best interests of the profession of our country and of the International Congress. Consequently, after giving due heed to suggestions from all parties, the Committee again assembled on September 3, in New York, and with much unanimity so altered the previously restrictive rule in regard to the American Membership as to do away with all delegations from other medical organizations, and to open the doors of the Congress to members of the regular medical profession in this and all other countries, with no other condition than that they enter their names on the register and take out tickets of admission. In other words, the conditions of membership were made essentially the same as in the preceding Congresses in London and Copenhagen. By the adoption of Rule 10 the Committee also made provision for an Executive Committee, not of its own members but of the general officers and presidents of Sections of the Congress itself, with power to take full charge of all the interests and management of the Congress as an inde-

pendent and self-sustaining organization, from the time of their appointment until the work of the Congress should be completed by the publication of its transactions. Having thus revised and adopted such rules as obviated all the objections hitherto made against the previously proposed conditions of membership, and provided for an independent organization of the Congress to be controlled and managed by an Executive Committee composed of its own chief officers, the Committee of Arrangements then selected the general officers and presidents of Sections, so far as was necessary to bring into existence the Executive Committee provided for in Rule 10, and by the adoption of a special resolution, transferred all the unfinished details of organization to the said Executive Committee, with full power to fill all vacancies, add a limited number to its own body, and to take full charge of the affairs of the Congress. The power to add to the number of the Executive Committee was given for the purpose of enabling a number of the most prominent of those who had withdrawn their support under proposed regulations essentially different, to accept places on the Executive Committee and again participate actively in all the interests and management of the Congress.

It is apparent, therefore, that the Committee of Arrangements, at their September meeting, deliberately and properly so shaped their action as not only to remove all the objections that had been urged against the rules as previously proposed, but also to open the way for all interests in the profession to again unite in sustaining the Congress as a true, independent, international organization, under the exclusive control of an Executive Committee composed chiefly of its own responsible officers. It only remains, therefore, for the present Executive Committee of the Congress to fill the vacant places in its own body by offering unreservedly several appointments to as many of the most prominent of those who had previously withdrawn as their places will admit, before filling any other important vacancies. If they accept, an end will be put to the dissentions regarding the Congress, without compromising the principles of any party. Can there be a reasonable objection to such a course of action!

DR INGLIS'S ADDRESS.

It is seldom that one has the opportunity of reading an address before a medical body which contains so much and so varied suggestion, as the Address of DR. DAVID INGLIS before the Detroit Medical and Library Association, published in this number of the JOURNAL. While it is in no sense a valuable contribution to our knowledge of any *one* department of medicine, and was not intended to be, it would be well, indeed, if each member of every medical society in this country could read and study it thoroughly, thoughtfully and carefully; for there are some valuable lessons to be learned from it.

The first lesson to be learned from it is—what has been and is often thought to be an impossibility—that a number of medical men can work together in peace and harmony, without wasting the time of the society and lowering themselves in the estimation of their fellows by quarrels and bickerings, usually of an entirely private nature. This opinion seems to be so thoroughly grounded in the minds of some members of certain societies that they do not seem to think any meeting of the society complete, or that the time for adjournment has come, until a certain number of epithets have been passed, and at least one committee has been appointed to investigate some one or more members of the society. In a few cases the real nature of a medical society seems to be forgotten, and the impression prevails that the reading and discussion of papers is an altogether secondary matter, the time for which is *after* the weekly, bi-weekly or monthly disagreement has been referred to a committee. The case of the Detroit Society may or may not be unique; but it is certainly interesting to know that ninety-five physicians *can* lay aside personal animosities in their society, and work together for a whole year without one instance of animosity or contention. "The great need of our profession is unity: no other profession allows personal animosity to so weaken its power as does our own."

Dr. Inglis refers to the large number of pathological specimens that have been presented at the meeting of his society during the past year; in itself a sign of a *working* society. It indicates, as he says, an increasing thoroughness of investigation on the part of the members. There can be no question that any paper that can be illustrated by specimens is far better if the specimens are presented. There is an amount of wholesome proof in a specimen that can never be obtained from a mere statement. And it is a matter that must ever be regretted that the laity look upon autopsies with such horror. Should the time ever come when they can be educated to the fact that such things are for the good of humanity, the millenium of medicine will not be far distant; for they will then be also educated to many other equally important facts.

From the Address we learn that set debates have

done much to maintain the interest and promote vigorous life in the Detroit Society. This plan might be adopted with obvious advantage by other societies. It will at least insure some carefully prepared remarks on one subject, as opposed to the frequent exhibition of careless remarks on many subjects. (It is not too much to say that medical society meetings are sometimes hopelessly tiresome on account of the disconnected and frequently valueless remarks of unprepared speakers. But this fact has already been pointed out in a recent editorial article in the JOURNAL, and need not be dwelt on further.)

The subject proper of the Address is embodied in the question, What is to be the future of therapeutics? In regard to our materia medica, Dr. Inglis thinks that the first thing to be done is to establish the place of action of the various drugs. "Of many physicians using the same general group of drugs one will achieve better results than the rest, simply by a more accurate judgment of the adaptation of the remedy." This, we think, sufficiently accounts for the amount of what we may call "therapeutic skepticism" among many physicians, of which our author speaks. It is scarcely possible to conceive of a physician who can judge of the adaptability of a drug, and who only uses it when it is indicated, who is skeptical as to its value. It is undoubtedly the fact that we should "more thoroughly work out the detailed symptoms of each disease." The man who does this, who discards the idea of "a pneumonia," or "a typhoid fever," and treats indications must be far from skepticism. And we cannot altogether agree with our author that "we have fallen lamentably behind in the study of symptomatology." It must certainly be admitted that much has been added to symptomatology within the past few years; the case of cerebral tumor so acutely diagnosticated by Hughes Bennett, and operated on by Godlee, is an apt illustration of this fact. At the same time, we must agree that the "only basis for a scientific therapeusis is to be found in an exact knowledge of two kinds of facts: *first*, what is the precise effect of a drug upon the functions of healthy organs; and *secondly*, what is the effect upon organs whose functions are already disordered?" Here we at once come to the field of Pharmacology, the importance of which was spoken of in this JOURNAL only a short time ago—and it is an importance which can scarcely be overestimated. And it is entirely proper to say that the only basis for a scientific therapeusis lies in a greater attention to Pharmacology; in a closer and a more general study of this branch of medicine.

In connection with the importance of a more thorough study of pharmacology, in order to establish a basis for a more scientific therapeusis, we may refer to that portion of the Address which speaks of the use of proprietary medicines. While it is a fact, and one to be deplored, that physicians use these preparations, a part of the fault at least must be laid at the door of their teaching. And while there is much to be said against the practices of many manufacturing pharmacists, there is also much to be said in favor of some. Merck, of Darmstadt, is a manufacturing chemist; but we much doubt if a dozen physicians in all Europe and America would go so far as to say that his methods are objectionable, or that it is improper to use drugs known by his name. Our strictures should be directed to the abuse, not the use, of manufacturing pharmacy. And it is entirely probable that many of the abuses even of "patent medicines" could be stopped if the State would take a little more interest in the health and lives of its citizens. But as one swallow does not make a summer, so one physician who prescribes proprietary or patent medicines does not represent the profession. As regards the unfairness of such prescribing to our own pharmacists, it may be said that if they would refuse to sell these goods they would not be sold to the extent that they are now. The small pharmacies are much more responsible for this state of things than are any members of the profession, both by selling these articles and by counter-prescribing.

Finally, much of the uncertain and unscientific therapeusis of the present day should be laid, we think, at the door of those who are so badly affected with the *cacoëthes scribendi* as to publish books of formulæ, in which one formula is given for the treatment of "pneumonia," another for "bronchitis," and so on to the end of the nosological list; as though a disease was always a determinate and definite entity, varying no more than a mathematical problem. These books seem to be written in order that the purchaser may use them as he would a dictionary. They are dangerous in the hands of a person who needs them, and those who do not need them never buy them.

TEMPERATURE OF THE ABDOMINAL WALL IN ENTERITIS OF CHILDREN.

PROFESSOR MONCORVO, of Rio de Janeiro, has recently communicated a very interesting note on this subject to the Academy of Medicine of Paris. It is now about three years since he commenced a series of investigations on this subject, and some of his conclusions were announced two years ago by one of his pupils: He had already applied Peter's method of thermometry of the chest to children, and his

satisfactory results induced him to ascertain its value in affections of the abdominal organs—chiefly acute and chronic enteritis. Dr. Moncorvo has exceptional facilities for work of this kind in his capacity as Professor of Diseases of Children in the Rio de Janeiro Policlinic. He makes no statement as to the number of cases examined, but his reputation as an acute clinician precludes any doubt as to the thoroughness or accuracy of his observations.

As is the case with most diseases in the Brazilian Capital, it seems, acute and chronic enteritis of children are very frequently complicated by malaria, or are of malarial origin; a fact which North American physicians can readily understand. Not only is this the case, but it is not at all infrequent that the malarial intoxication in children seems to be localized in the intestinal canal, and that the diarrhœa and intestinal symptoms are so predominant as to mask whatever other symptoms may be present, and may persist long after all other signs of malarial intoxication have disappeared. This being the case, there must necessarily be more or less difficulty in making correct thermometrical observations; since any change in the temperature due to the malarial poison itself must be excluded in taking account of changes due simply to the intestinal affection.

Having found by means of a Casella thermometer (in which the reservoir of mercury is encased in wood, so as to be beyond the reach of external causes of error), that the normal temperature of the abdominal wall of young children varies between 95° and 98.8° Fah., according to the age of the subject and time of observation, he classified the patients into two groups: in the first were placed those cases of enteritis in which there was an elevation of rectal or axillary temperature—febrile cases; in the second were placed the non-febrile cases, in which there was only a greater or less degree of diarrhœa. In the first class of cases the temperature was always very high, the average being 100.2°. And as the increased local heat is directly dependent upon the elevation of the central temperature, it is easy to draw conclusions as to the influence of the intestinal affection. In the second class of cases abdominal thermometry may also be of great value. Moncorvo states that in the greater number of these cases he has been able to note the progress of the intestinal affection by close attention to the thermometric curve. His observations show that the surface temperature of the abinal wall rises a little before the diarrhœa sets in. On the other hand, he has found that the thermometer still recorded a temperature of 97.4° when the diarrhœa was said to have disappeared; but investigation would show that the stools were not yet normal. The intestinal mucous membrane has not yet regained its physiological condition, and the fact is shown by the thermometer.

By this means, therefore, we are in a condition to give a much more certain prognosis, and to know the date of termination of the enteritis. During the entire course of the disease the local temperature is more or less elevated according to the severity of the case; the temperature ordinarily varies between 97.4° and 98.8° That the local temperature decreases precisely in accord with the declination of the intestinal affection is a further proof of the value of local thermometry in these cases. As the alvine evacuations decrease day by day under the influence of proper treatment, the local temperature falls in a direct and constant ratio.

SOCIETY PROCEEDINGS.

PHILADELPHIA PATHOLOGICAL SOCIETY.

Stated Meeting, October 22, 1885.

THE PRESIDENT, J. C. WILSON, M.D., IN THE CHAIR.

DR. J. COLLINS WARREN, of Boston, read a paper on

A COMPARISON OF THE CHANGES IN ARTERIES AFTER LIGATURE, AND IN THE DUCTUS ARTERIOSUS AND UMBILICAL ARTERIES AFTER BIRTH.

After the ligature of an artery in continuity, the earliest changes noted are the formation of the thrombi within the vessel and the development of a mass of inflammatory tissue or callus around the point of ligature externally. No perceptible cell action can be observed in the inner wall with low powers during the first week, although under favorable circumstances a proliferation to a limited extent of the endothelial cells near the point of ligature can be seen with high powers, and occasionally a few wandering cells may be found to have penetrated the walls of the vessel at the same point. In the second week, the bundle of fibres of the adventitia, which were surrounded and held by the knot, have been absorbed, and the two ends of the vessel retract slightly from one another, leaving the ligature imbedded in and partly disintegrated by the granulation cells. The walls of each portion appear to form a complete cul-de-sac, and it looks at this time as if the healing process were complete; but it can hardly be said to have more than begun, as the vessel has not yet passed through what may be considered as the first stage of healing.

The beginning of the second stage is marked by an unfolding of the ends of the vessel, the walls separating somewhat after the manner of opening of a bud, which permits the entrance of a considerable quantity of the granulation tissue. A disintegration

of the thrombus follows, and we have now a fully developed external and internal callus, a small fragment of clot still protecting the latter from the current. The ligature may be totally disintegrated and absorbed, or it may have become encysted, or finally it may have created a small abscess about itself which has discharged the fragments of thread through a sinus opening externally. The second stage is completed when the internal growth has reached the neighborhood of a branch. After this we have an absorption of the callus, which, as in fracture, is only a provisional structure, and eventually the two ends of the vessel are found held together by a slender cord of varying length. The walls of the vessel are slightly separated at each end by a a cicatrix consisting of connective tissue externally, inside of which is another layer consisting largely of unstriped muscular fibre, the surface being covered within by a new endothelium. The cicatrix is always pierced by a vessel which terminates in a number of capillaries ramifying in the cord. We have here a scar made up of three layers resembling closely the three coats of the vessel. In the large vessels of amputation stumps we have a somewhat different series of changes. Soon after the ligation, the end of the vessel may be seen imbedded in granulation tissue, and containing a thrombus of varying length. By the second week there is a marked change in the intima extruding for some distance above the point of ligation, probably to the first large branch or to the origin of the vessel.

Examined several months later, when the healing process has been completed, the vessel is found to be preserved in the form of a cord running from the first large-sized branch to the cicatrix of the stump; on laying open this cord the walls of the vessel are found preserved, the interior being filled with new tissue, leaving spaces occupied by one or more vessels. There has been a process resembling that known as obliterating endocarditis, by which the calibre of the blood channel has been narrowed to an extent to adapt it to the diminished supply. In this obliterating tissue we find comparatively large vessels with new cords consisting of the endothelium, an elastic membrane, and also a new media. An erosion preparation would best represent the condition of the arteries of the stump at this time. The main artery would, after giving off its largest branches, break into a spray of smaller vessels, no one of which would predominate.

A comparison of these two modes of healing with the changes in the arterial system seen after birth shows certain resemblances in the two processes. The ductus arteriosus, about the time of birth, differs considerably from the structure of the aorta and pulmonary artery. The media is much thicker than in either of these vessels, it is thrown into irregular folds which are increased at the time of birth, and help to narrow its calibre. The distinctions between the different layers of its wall are less marked than in other vessels. The lamina elastica is indistinct and in places apparently wanting; the media consists chiefly of longitudinal layers of muscular fibres, a few circular bundles existing in the innermost layer. A few weeks after birth, a greater portion of the walls of the ductus undergoes hyaline degeneration, the inner or circular fibres of the media alone remaining, these being reinforced apparently by a growth from the media of the larger vessels. At this time there is an active growth of long spindle-shaped cells, with staff-shaped media, at the edges of the media bordering on the opening into the aorta there is also moderate thickening of the intima. Eventually the hyaline tissue becomes absorbed and is replaced by a ligamentous band of fibres, which becomes continuous at either end with the media of the larger vessels. At the aortic end, in a longitudinal section, we see the media slightly separated. At the point of the cicatrix, and between them and also continuous with them, are the longitudinal fibres of the ligamentum arteriosum. In this ligamentous tissue and between the edges of the media are numerous new muscular cells; nearer the surface is a layer of thickened intima, which in the aorta has not only connective tissue in it, but also a deep muscular elastic layer; in the centre of the depression marking the site of the cicatrix a small round vessel is given off, which penetrates into the axis of the ligament, where it either loses itself in the capillary network or becomes continuous with a similar vessel coming from the pulmonary artery. We have here conditions closely resembling those which have been described as existing in the cicatrix of a large artery after ligature in continuity, namely: the slightly separated ends of the media, between which lie the fibres of the ligament connecting it with the pulmonary artery, a new intima and a new growth of muscular cells, and finally a central arteriole. The only point of difference is the preservation of a layer of circular muscular fibres which form an outer wall to the ligamentous band, a much needed support at a point exposed to great tension. Further protection is afforded by the oblique insertion of the ductus into the aorta, diminishing the pressure upon this particular point. At birth the umbilical artery, or that usually called the hypogastric artery, the portion within the abdomen, at its origin from the internal iliac is a vessel of considerable size, being, in fact, a continuation of that artery. At its termination in the umbilical wound it has greatly contracted, and is filled with clot for a distance of about one inch.

The outlines of the various walls are not as distinct as in other vessels, and the elastic lamina for the most part wanting. No special change is seen in the elements of the walls of the vessels, except a slight accumulation of endothelial cells near the apex of the thrombus. A few weeks later, there is a distinct growth on the inner surface of the wall up to its point of origin, the terminal portion having undergone a hyaline degeneration and obliteration for a short distance. The vessel has greatly contracted throughout its entire length, and its calibre is further diminished by the growth in its interior. Cross sections taken from the artery in adult life show the media as a wall thick out of all proportion to the size of the vessel, and consisting not only of its original wall, containing longitudinal muscular cells interspersed with elastic tissue, but also an inner

circular row of cells which is provided with a well formed elastic lamina. It seems probable that the greater portion of the hypogastric artery has been preserved, the ligamentous band which extends to the umbilicus consisting of the obliterated extremity of that vessel much elongated during the process of growth. The series of changes which occur in the hypogastric artery after birth is closely analogous to that seen in the main trunk of an amputation stump —a slight portion of each vessel is destroyed, both retract and are attached to the terminal cicatrix by a band of fibrous tissue, both remain as pervious vessels with thickened walls and narrowed calibre. In both the process is not unlike that seen in the so-called obliterating or compensatory arteritis. Arteritis hardly seems a term applicable to the changes taking place in normal arteries after birth, nor can the alterations which have been developed through the whole length of a large vessel extending a considerable distance from the seat of inflammation, be strictly regarded as of an inflammatory nature. May not the obliterating growths found to exist simultaneously in terminal arteries in widely removed portions of the body of the same individual also be regarded not as of inflammatory nature, but rather as a secondary and formative process, closely connected with disturbances in the mechanism of nutrition, designed to adapt the vessels to a diminished blood supply?

DR. S. W. GROSS said that, in view of the fact that Dr. Warren's teaching seemed so distinctly opposed to that of other observers, he would like to have some points cleared up. He would, therefore, ask if Dr. Warren held, 1st, that instead of the external coat where it is included in the ligature, sloughing and coming away with the ligature, the pressure of the ligature sets up an irritation which causes the adventitia to return to its embryonic state, with a reconversion, after the ligature has cut through this, to connective tissue; and, 2d, whether the repair of arteries was brought about by the ingrowth of the cicatricial tissue, which he likened to provisional callus, together with some proliferation of the muscular cells of the media?

DR. RANDALL said that, as the result of careful study of the subject, he had always found the clot present at first, and that it was honeycombed even in the first few days by the contraction of its fibrin, through the spaces thus formed the blood seemed to be circulating. Cells, either original white cells or of endothelial origin, occupied the walls of these cavernous spaces and seemed to sheathe them. The red cells early melted down into a homogeneous mass, furnishing the framework upon which the reparative tissue was built. The "plastic clot" of some observers he had not seen, the new cells being distributed throughout the old clot as well as upon the vessel walls, and not specially collected in the immediate neighborhood of the ligature. The obliteration of the vessel was accomplished by the growth and contraction of the trabeculæ of new tissue built upon the remains of the original blood clot. Even close to the ligature he had not found the vessel walls greatly altered, merely showing a great increase in the nuclei and in the number and size of the vasa vasorum, the lamina elastica being distinct, and, as a rule, intact. Toward the end of the first month the new tissue was largely spindle cells, but careful staining had given no suggestion of muscular tissue. But, not having carried his study beyond the first month, he had no data upon this subject, since the development of muscular tissue is claimed to occur only at a later stage.

DR. H. F. FORMAD was inclined to favor the views of Dr. Randall. The observation of Dr. Warren that the new-formed connective tissue played the most important part was to him entirely novel. He thought probably the pressure upon the artery from without, with consequent diminution in its lumen, might bring about a condition more analagous to the growth of fœtal life than to inflammation.

DR. MEARS remarked that we are liable to be confused in considering this subject by the presence of the blood in the vessel. So far as the process of healing after ligature is concerned, we may consider only the structures which enter into the formation of the vessel wall, which is complex in character, and composed of connective, plastic and endothelial tissues. Dr. Warren has given us a very clear demonstration of cicatrization as it occurs in these structures after application of the ligature.

DR. TYSON said the most novel feature to him in Dr. Warren's observations was the part played by the muscular tissue. This apparent increase in the muscular tissue he was inclined to believe was really only a proliferation of intermuscular connective tissue. He was becoming more and more convinced that there was only one kind of inflammation—the interstitial. He feared that he might himself mistake a proliferating connective tissue with spindle cells for muscular tissue. It is true that new-formed capillaries and arterioles have muscular walls, but the development of these seemed to him to be a slower process. He had expected to find a process of repair in the ductus arteriosus different from that in an ordinary artery. This process he had expected would be a true endarteritis, for it seems that the conditions of closure here are rather different from those in the ligaturing of an ordinary artery and in the umbilical artery, where we have also ligation practiced.

DR. WARREN, in closing, said in cases where there is much breaking down and little repair, there might be a sloughing of the external coat, yet, in his experiments, this part of the arterial wall did not slough, but was simply absorbed by the granulation tissue, as is the ligature itself in some cases. In the normal condition even, the elastic lamina is not a perfectly continuous plate; he did not refer to these breaks, however, but to numbers of little ruptures incident to the pulling out of the wound of an artery in the act of ligature—however, this need not necessarily occur. He had not attempted to identify anything like muscular tissue early; this could not be recognized till we had a permanent cicatrix formed. He had carefully eliminated all sources of error, and it seemed to him that in almost all cases there was a considerable number of these muscular cells. The layer was not always as well marked as in his dia-

grams; especially was this true of human cases, but these had all been in alcohol for some years; of the fresh specimens he had selected only those in which he had considered the process complete. In a specimen which he had here to-night, through an opening in the lamina elastica cells could be traced from the muscular layer, those in the inside resembling exactly those outside. These facts, together with the fact that we have normally a few muscular cells inside of the lamina elastica, lead him to believe in the proliferation of the muscular tissue. The disintegration of the blood clot is accomplished by granulations growing inward from the callus, there being two sets of blood vessels, one in the granulations, the other, (blood spaces, rather) in the clot itself; these unite about the end of the first or second month.

Stated Meeting, November 12, 1885.

THE PRESIDENT, JAMES C. WILSON, M.D., IN THE CHAIR.

DR. FUSSELL presented

SPECIMENS FROM A CASE OF MASTOID DISEASE.

The patient was a male, æt. 25 years. Never had scarlet fever. Since vaccination at the age of 5 years, has had occasional attacks of suppuration of the middle ear. General health was good. Was first seen by Dr. Fussell July 14, for several weeks he had had earache, with one slight chill. When seen hearing was very poor, membrana tympani opaque, swelling and redness over left mastoid process and temperature elevated. His general condition grew worse, and an incision was made over the mastoid process, but it failed to find pus. On extending this incision a few days afterward, pus flowed freely, and he was much relieved. He afterward grew worse, fell into a typhoid condition and died July 24. Before death, blood oozed freely from the cutaneous surface. At the autopsy, the mastoid cells were found filled with pus; in the inner half of the process was a large irregular cavity filled with a pultaceous mass of necrosed bone, this communicated with the middle ear. The walls of the lateral sinus were thickened, easily detached from the bone, and the seat of a purulent inflammation. The sinus contained no thrombus. The meninges over this spot were discolored, though not inflamed, but there was a patch of inflammation over the anterior edge of the left lobe of the cerebellum. Brain substance was normal. Remaining organs were not remarkable, except the right lung, which contained in its apex two small abscesses. From the condition of the sinus, the reporter thought there had been an actual admixture of pus with the blood.

DR. JOHN H. PACKARD presented

AN ANOMALOUS LUNG.

It was taken from the body of a negro who died of Bright's disease. From the inferior surface of the lower lobe of the right lung springs a tongue-shaped process of pigmented, crepitant lung tissue two and one-half inches long and two inches wide at its base, resting upon the diaphragm, its upper surface being in apposition with the under surface of the lower lobe. Dr. Packard had recently seen in the body of another negro a similar anomaly, except that the process was smaller and situated more anteriorly.

DR. J. H. MUSSER presented a

CYSTIC KIDNEY.

It was the right kidney, and was taken from the body of a man, æt. 72 years, who died of apoplexy. The kidney was cirrhotic and contained two cysts, the larger occupying the upper one-fourth of the organ. Its walls were firm, and it contained clear fluid in which floated cheesy masses, which the reporter thought were degenerated pus. When first seen, the patient was passing small quantities of bloody, highly albuminous urine, and complained of pain in the right renal region. These symptoms were apparently due to an acute process grafted upon the chronic lesion. The blood and most of the albumen disappeared, but the pain persisted. Was this pain due to the cyst?

DR. MUSSER presented also a

CARCINOMA MAMMÆ

removed from the body of a woman, æt. 75 years. The breast had been injured eight years ago, and three years afterwards the tumor noticed. There was never any pain in the tumor. The lymphatic glands of the axilla were involved. In addition there was a large lipoma of the back of the arm. She had frequent attacks of severe pain in the tibia and left parietal bone, apparently due to periostitis, and not to any secondary growth. There was no syphilitic history.

DR. MUSSER also presented

SPECIMENS FROM A CASE OF DIABETES MELLITUS.

The patient was a female, æt. 43 years. Had a vesico-vaginal fistula for fourteen years. Diabetes had persisted, without apparent cause, two months. Patient was unusually fat, and had lost no weight. She died of coma. Temperature in the abdominal cavity three hours after death was 107.8° F. Liver large and fatty; gall bladder contained thirty stones. Kidneys fatty. Pancreas normal. Blood had a most marked lactescent appearance, and after standing twelve hours globules of fat collected on its surface. Lacteals in the mesentery engorged with chyle. Microscopic examination of lungs showed no fat emboli. In the urinary bladder was a large phosphatic calculus. Dr. Osler thought the peculiar condition of the blood was what we should find normally in a person dying during digestion, and called attention to the fact that very frequently in diabetics there is the engorgement of the lacteals.

DR. MUSSER also presented a

FŒTUS PAPYRACEUS.

The mother was delivered in the morning of a mature living child, and in the evening of this fœtus. It is apparently of the fifth month of pregnancy, and with the exception of shriveling and paling of the skin, is quite normal. The cord is thin and soft, its length unknown. The placenta is thin, flat, compact, and whitish-yellow, apparently having undergone complete fatty degeneration.

DR. JOHN B. ROBERTS presented a

SMALL TUMOR, THE SIZE OF A HICKORY NUT, REMOVED FROM THE BACK OF THE WRIST OF A YOUNG MAN.

It had the clinical appearance of an ordinary ganglion, but attempted evacuation showed it to be solid. It was then enucleated and found to have been developed within the theca of the tendon going to the middle finger, and probably between its fibres. The great rarity of solid tumors in this locality was mentioned.

DR. WM. OSLER asked if the patient had a rheumatic history, although this specimen was rather large for a subcutaneous nodule occurring in rheumatism.

DR. ROBERTS knew nothing of the history.

DR. C. B. NANCREDE presented *Fluid from an Encysted Hydrocele*, which fluid contained large numbers of dead spermatozoids.

DR. W. E. HUGHES presented a

PRIMARY CARCINOMA OF THE LIVER.

Female, æt. 58 years. A daughter died of cancer of the uterus. She had been in good health till 18 months ago, when flesh and strength began to fail. With this there were occasional attacks, lasting about a week, of headache and sick stomach, followed by diarrhœa. There was pretty constant lancinating pain in the hepatic region. Five months ago she had an attack of jaundice (her only attack), lasting two weeks, and at this time a humor was detected in the liver. At the autopsy the liver was found much enlarged, and scattered through its substance were firm cancerous nodules varying in size from an orange to a pin head. In addition to these there were several cysts, one in the left lobe two inches in diameter, filled with clear fluid. The liver substance between the cancerous nodules was normal. The gall-bladder was full of healthy bile and the ducts patulous. There were no enlarged glands in the fissure of the liver, but the retro-peritoneal and mediastinal glands were increased somewhat in size, and the seats of secondary deposits; with these exceptions these was no growth outside the liver. The intestines were crowded into the left side of the abdomen by the enlarged liver. The stomach was very small, only one inch in diameter at its fundus. There was an intussusception, three inches in length, at the ileo-cæcal valve, which, on being reduced, which was effected with some difficulty, showed the opposed surfaces of peritoneum covered with lymph.

DR. H. F. FORMAD presented specimens and read a paper on

AN ANALYSIS OF 250 AUTOPSIES ON DRUNKARDS, ILLUSTRATING THE MOST PROMINENT ANATOMICAL LESIONS OF CHRONIC ALCOHOLISM.

He considered the most conspicuous lesions to be cyanotic induration of the kidneys, fatty infiltration of the liver, and mammillated stomach. His cases had been those in which there had been a history of a long-continued series of debauches, the subjects often dying in one of these debauches, and did not include moderate drinkers or those who perished after imbibition of an enormous quantity of alcohol without any previous chronic causes. He thought that the exposure, irregularities of diet, etc., incident to a state of drunkenness, had much, probably more than the alcohol itself, to do with the production of the lesions, but it was not at all possible to separate one from the other. He gave a long list of lesions considered by various authors to be results of chronic alcoholism, among which the cirrhotic liver, with contraction, held a prominent place. He had had himself at one time considered cirrhosis a very frequent, if not almost necessary, concomitant of long-continued excessive use of alcohol, and had even testified in court that a certain person was not likely to have been a hard drinker because at the autopsy no cirrhosis of the liver was found. He had thought, too, that the connection between the two was so close that it was impossible to have a case of cirrhosis without a previous history of alcoholism, as is held by various authors, Therefore, it was surprising to him to meet in his 250 autopsies with only six cases of cirrhosis of the liver with contraction, In 220 cases the liver was considerably or even very much enlarged, the enlargement in most cases proving to be due to a fatty infiltration. Cyanotic induration of the kidney, and chronic gastritis with mammillation of the stomach, were found in nearly every case. This cyanotic induration is peculiar, and differs from the cyanotic induration due to heart disease. At a future time he will give a detailed account of the above lesions, and a more extensive analysis of the cases.

DR. JAMES TYSON could not speak from a systematic observation of a large number of autopsies in the cases of confirmed drinkers, but he remembered distinctly being surprised, in several cases, by the absence of cirrhosis where he confidently expected to find it.

DR. WILSON said that Anstie, in the article on alcoholism in "Reynold's System of Medicine," had called attention to the comparative infrequency of contracted liver in confirmed drinkers. This observer, in an extensive out-patient practice in London, had seen large numbers of cases of alcoholism, but very few among them presented the physical signs of cirrhotic (contracted) liver. The experience of the staff at Blockley Hospital confirms this view. There many of the patients are soaked with alcohol, but even among those whose death is directly or indirectly due to alcoholic excess, fatty liver is much more common than contracted liver.

DR. OSLER thought the experience of pathologists and morbid anatomists with histories of patients not of the most satisfactory character, he often having had cases to dissect where he knew very little of the history. Before saying these cases were chronic alcoholics Dr. Formad should present more specific statements about them. His own experience with livers, in a large number of autopsies on cases of chronic alcoholism, had led him to divide them into four classes: 1. Those in which the condition of the liver is pretty satisfactory; some of these cases may take alcohol for many years and yet the liver pass muster. 2. Fatty cirrhotic livers; the cirrhosis may

not, perhaps, be distinct to the naked eye but plainly shown by the microscope; this is the largest class. 3. Hobnail livers; these he would say were much more common than in Dr. Formad's series. 4. Hypertrophic cirrhotic livers. The difference between his observations and those of Dr. Formad might possibly be accounted for by a difference in the form of alcoholic beverage taken. He had not observed the special form of kidney described by Dr. Formad. In reply to a question, he said in order of frequency he would place them fatty cirrhotic, hobnail, hypertrophic cirrhotic, apparently normal.

Dr. S. Solis-Cohen said that there were certain theoretical considerations which suggested themselves in this connection. The text-books teach that the lesions of alcohol are of two kinds, sclerosis and steatoris. It is known that in some organs the fibrous change precedes the fatty one. The latter is the higher grade of degeneration. The subjects of Dr. Formad's autopsies were confirmed whisky-soakers, in whom one would expect to find more intensity of degeneration than in those whose use of alcohol, though persistent and excessive, was not so outrageous. Another point which had not been alluded to was the fact that some lesions might result from a local action of the poison upon the tissues, while others might be due to its systemic action. No study of the subject could be complete in which these points were overlooked.

Dr. B. A. Randall suggested that the point touched upon by Dr. Osler—the character of alcoholic beverage—might be very important. In Vienna, among beer drinkers, he had found the fatty liver much more common than the cirrhotic, while in England, where much gin is drunk, and he should suppose in Scandinavian countries, where they drink altogether strong spirits, the cirrhotic liver is doubtless comparatively frequent.

Dr. Musser had recently had to go over the records of the Pathological Society, especially in liver diseases, and had found the total experience of different observers the same as Dr. Formad's, and also in those cases cirrhosis was caused not so much by heavy drinking as persistent drinking of spirits on an empty stomach.

Dr. Formad presented the

SAC OF AN EXTRA-UTERINE PREGNANCY.

The woman from whom this was removed had not suspected that she was pregnant. She was in perfect health until twelve hours before death, when she was suddenly seized with excruciating pain in the lett groin, rapidly followed by collapse. On opening the abdomen it was found to contain at least a gallon of partly clotted blood. About the middle of the left fallopian tube was the sac with a rent in its posterior wall. This sac was one inch in diameter and contained clotted blood and placental tissue. The uterus was twice its normal size. The fœtus was not found.

Dr. Formad also presented

AN ANEURISM OF THE ASCENDING AORTA RUPTURING INTO THE PERICARDIUM.

The patient was a laboring man and had considered himself in perfect health. He died very suddenly. The aneurism, half an inch in diameter, was situated just above the posterior aortic leaflet and had broken through the wall of the aorta at the point where it touches the descending cava. The cavity of the pericardium was fully distended with clotted blood.

OBSTETRICAL SOCIETY OF PHILADELPHIA.

Stated Meeting, Thursday, November 5th, 1885.

The President, B. F. Baer, M.D., in the Chair.

Dr. John M. Keating, M.D., read a paper on

LYMPHATIC LEUKŒMIA IN CHILDHOOD.

The short paper which, by invitation, I propose to read to you this evening will, I am sure, prove of interest on account of its clinical rarity and the infrequency with which we meet the disease in current literature. To make the subject more interesting and more lucid, at the risk of recalling to your memory matters already familiar, I will premise with a few remarks on the blood in health in children and briefly with its diseases.

There is a uniformity of composition of healthy blood which is curious. This is brought about by that equilibrium which is so striking in textures regulated by the production and waste. The blood corpuscles which we are called upon to study are: 1. The red, of which there are floating in the plasma about 5,000,000 to the cubic millimeter. These are about 1-3200 of an inch in diameter. 2. The white, of a diameter of 1-2500 of an inch, having the proportion of 1 to every 300 or 500 red. 3. The neucleated red found in the fœtus and infant, and disappearing about the third or fourth year of life. These may contain one or more nuclei in some instances protruding from the cell. They measure from 1-1400 to 1-2000 of an inch. 4. The *hemato-blasts* of Hayem—"Small discoid, colorless, corpuscula, normal constituents of healthy blood—in drawn blood they aggregate in clumps known as Schultze's granule masses." As to the origin of the red corpuscles I will quote: "They are developed from colorless corpuscles, the lymph-cells or leucocytes. The nucleated corpuscles of the embryo also aid in the development. These nucleated cells disappear early in childhood, and are therefore only in the red marrow." It is Osler's opinion that "they apparently originate from colorless marrow-cells which gradually become more homogenous and hemoglobin develops in the protoplasm, the nucleus degenerates and disappears, when the cell has the appearance of an ordinary red disc." Possibly these nucleated cells may give birth to red cells by the process of budding. The relations of the cytogenetic organs to blood formation has always been somewhat debatable ground; the present state of our knowledge may be formulated about as follows: "The spleen certainly takes part in the development of colorless corpuscles, but its participation in red blood formation is more doubtful; though the opinion prevails widely that the spleen is one of the important organs in the form-

ation of red corpuscles, the evidence for this belief is of an exceedingly scanty nature.

"The lymphatic glands and adenoid tissues in other regions are the seat of constant production of colorless corpuscles, but of their relation to the red corpuscles there is the same lack of information as in the spleen."

Neumann and Bizzozero pointed out the fact that the red marrow appears to be the seat of blood formation. In the adult it is the only region in which embryonic or nucleated red cells are found. In the young the marrow fills the long bones. It is Osler's opinion, and I have quoted from his recent admirable articles on this subject, that the evidences of the development of red corpuscles in the marrow rests upon the constant presence of nucleated cells infiltrated with hemoglobin, and of their fusion. In excessive hæmorrhage, natural or induced, it appears to undergo active proliferation, and it is an interesting fact to notice the marvelous rapidity with which the red corpuscles are reproduced after a hæmorrhage. The amount of hæmaglobin in healthy blood, according to Preyer, is 13.45 grammes to 100. This relation is important for us to have in mind, as it proves an index to treatment. The color test being used, I feel certain that before long it will be a matter of more than ordinary interest to the general practitioner when the means of applying the color test shall be generally adapted, as has been done already to daily practice. In the new-born the blood is said to amount to one-tenth part by weight of the body. In the adult, one-twelfth to one-fourteenth. I may also note that it has been shown by Neumann that the liver in the embryo may be the seat of the formation of corpuscles, though in the adult it is the seat of their destruction.

Let us now take up the subject of anæmia in children. Were I to attempt to do justice to this condition, it would take far more time than your patience would allow. We will omit that caused by hæmorrhage, by toxic agents by mineral poisons, by miasm, by syphilis, in which we have an increase in the watery elements, and a diminution in the albuminous, and consider that dependent upon disorder of the blood-making organs themselves, and here we meet with a most difficult problem. By the blood-making organs we understand the spleen, the lymphatics tissues and the bone marrow, remembering of course that some of their tissues have also to do with blood destruction. We, however, definitely know that an increase in the cytogenetic tissues is associated with disturbances in blood formation. The organ undergoes a hyperplasia, particularly of its fibrous constituent, and the marrow of bone changes from normal appearance to one like spleen-pulp. The blood of all patients suffering from anæmia presents a reduction in the number of red-corpuscles. This is true, no matter whether the seat of the trouble is located in the spleen, the marrow of bone, or in the general lymph glands; the white cells may or may not be increased; the clinical features of a case of anæmia will be strikingly alike, let the cause be in any one of the blood-making organs, all the more important symptoms will be present. To quote once more, such common features would be "the progressive anæmia with its group of circulatory symptoms, the irregular febrile reaction, essential fever of anæmia, the absence of marked emaciation, the tendency of serum, the progressive debility, the recurrence of hæmorrhages, gastric and intestinal, gastric and intestinal disturbances, and the resistance to treatment." These affections that have so many symptoms in common are grouped as distinct diseases under the following heading, viz: leukæmia, Hodgkin's diseases (anæmia lymphatica, splenic anæmia, and idiopathic anæmia.)

Leukæmia signifies a hyperplasia of the blood-making organs with anæmia and an increase in the colorless corpuscles. Of this form we have three varieties, the splenic, lymphatic and medullary. Leukæmia is present at all ages; the youngest case recorded by Osler being an infant of eight months. The chief symptoms are insidious onset, anæmic appearance, bleeding at the nose or other hæmorrhages, frequent diarrhœa or other gastro-intestinal disturbance. The spleen is enlarged, gradually increasing in size from the onset, and finally it may interfere with the circulation and cause difficulty of breathing by pressure. Late in the disease the liver is also enlarged. The lymph glands in most cases are affected and sometimes slightly enlarged. The tonsils and follicles of the phanynx are usually enlarged. The lymph glands of the intestines and of the peritoneum are always enlarged. Fever is present and increases as the case progresses, and is usually of the remittent type. But the most important aid to differential diagnosis is the microscopical examination of the blood. This I give in detail in the case that forms the basis of this paper which I will now relate. I was called in consultation by Dr. J. W. Gadd, of this city, to see the child with him the latter part of last month, and the following notes were given by Dr. Gadd.

Mamie McC., aged 4½ years, had measles when about 2 years old, and from which she recovered without complication or sequelæ. About August, 1884, the tissue surrounding the eye became much inflamed and swollen. The swelling increased so much that the eyelid could not be opened for several days. After continued poulticing an abscess formed and broke, discharging a quantity of pus and continued to do so for a considerable time, but finally healed up with small scar. The child never complained, yet was pale and did not want to play as other children did. I believe this was more due to her disposition than to the effect of any disease. About midsummer there appeared a rash, all over her body very thick, and resembling the eruption of measles; as it matured it was crowned with small white caps or heads. The epiderm soon came off in large patches. The child had no fever. As the eruption faded the mother observed purplish spots, like bruises, making their appearance. These were considered by the parents to be bruises due to falls. No attention was paid to her condition until September 28th, when I was called in the evening to arrest an epistaxis which had existed most of the day. I found the child lying on a sofa, though able to sit up, with blood slowly trickling from the nose, each nostril contained a large clot. The child appeared very anæmic, with slight fever,

yet did not complain of anything except weakness. The mother stated that the appetite had been very poor for some time past. The bleeding from the nose was very easily arrested by removing the clots and packing with a strip of lint in each nostril. I also gave the following internally.

℞ Acid gallic..... gr. xxx
Acid sulph., dil..... m xl
Ext. Ergot, fl..... m xxx
Syrupi..... ℥j
Aquæ q. s. ad..... ℥ij M
S. 1 teaspoonful in water every hour.

Also ordering her as much milk as she cared to take, with the precaution that she should sip it slowly. Tuesday morning I saw her and found her in the same condition, except that the epistaxis had been arrested. I thought it best not to remove the lint packing. It now being daylight, her mother called my attention to the bruise-like spots over her body. There were in size from that of a two-cent piece to that of a fifty-cent piece, and two of them, situated one over each trochanter, were as large as silver dollars; with the exception of these two they were all, I believe, situated over the soft parts, such as over the belly of a muscle, and varying in color according to age, from a bluish-black to a greenish-yellow. She still had some fever, and her heart was more rapid in its action than normal; hence I gave her, in addition to the gallic acid and ergot mixture, the following mixture:

℞ Liq. potass. cit..... ℥j
Spts. æth. nit..... ℥ij
Tr. aconiti rad..... gtl xv
Syr. limonis..... ℥iv
Aquæ q. s. ad..... ℥ij M
S. 1 teaspoonful every two hours.

Wednesday morning I found her feeling somewhat better, though still having slight fever, pulse 124 per minute and moderately weak in character. I did not detect any abnormal heart sounds. I renewed the lint packing without any further bleeding and with much relief to the patient. I then ordered tr. digitalis in three drop doses every three hours, and also the following mixture:

℞ Quin. sulph..... gr. viij
Tr. ferri citro-chlorid..... ℥j
Syr. tolu..... ℥j
Aquæ..... ℥vij M
S. 1 teaspoonful every three hours.

Thursday I found the patient, to use her own language, well; evidently much better. Fever had entirely subsided; the heart's action remained abnormally rapid. The cervical glands were slightly enlarged, but no enlargement of the tonsils and apparently no inflammation of the fauces. Treatment was continued with the addition of more nourishing food, beef-tea, wine-whey, etc. The child seemed so much better that I said it might come to my office the next morning instead of my going there. Later in the same evening she took a sudden change for the worse, but I was not sent for until Friday morning. Now the patient was suffering from high fever, 104° F. in the axilla, pulse 134 per minute, compressible. The cervical glands much enlarged and very hard; the bowels had not been moved for twenty-four hours; the tonsils were but very slightly swollen; there were no patches on the throat. Thinking that possibly she was developing a malignant form of diphtheria, I at once put her on the calomel treatment until the bowels were moved, giving her three grains repeated in two hours, and then two grains, when the bowels were moved freely and the calomel stopped. I also gave suppositories of two and a half grains of quinine every two hours; also

℞ Liq. ammon. acet..... ℥j
Spts. æth. nit..... ℥ij
Syrupi..... ℥iv
Aquæ..... ℥ij M
S. 1 teaspoonful every two hours.

I continued the tr. digitalis in five drop doses every two hours. I also applied hot flax-seed poultices to the enlarged glands, and at noon plenty of beef-tea and milk, but the child did not care for food, and it was difficult to get her to take any nourishment. At mid-day I noticed for the first time—although I had carefully and frequently listened before—a systolic heart-murmur. The temperature was but little affected during the night and next morning (Saturday) at 7:30 I found it as high as ever, 104° F. in the axilla. Fearing that endocarditis had set in, from the continuance of high fever and the heart-murmur, I at once applied a mustard plaster to the pericardium, followed by a poultice, giving internally potassium iodide and ammonium carbonate, continuing the digitalis until noon, when I met Dr. J. M. Keating in consultation.

Physical signs showed, in addition to what has been mentioned, an enlargement of the spleen, yet there was no history of malaria. Dr. Keating did not think that endocarditis had set in, believing the murmur to be rather of a hæmic character, yet as a stimulant to the heart suggested the application of a blister; internally, very small doses of Basham's mixture every three hours, with the free use of alcohol, beef juice in small amount, etc. The temperature to be kept down to 102° or lower by means of the wet sheet. On examining a specimen of the urine which had just been passed, and the first that I had been able to obtain, I found it to be of sp. g. 1016, of a light straw color, free from albumin and sugar. The child could not retain either medicine or beef juice. The nose again commenced to bleed, to prevent which I again plugged the nostrils. Soon large clots of coagulated milk were vomited, the result of having been given by half-cupfuls at a time, which was entirely contrary to my direction. Her stomach soon became settled and she took brandy and crushed ice in small quantities. We wrapped her up in a wet, sheet and then poured cold water over her until the temperature came down to 101° in the axilla, which required about thirty-five minutes. She was then wrapped in a blanket. In two hours the temperature was again 104°. We gave several of the wet packs during the afternoon and evening, and, notwithstanding we were at the same time giving 2½ grains of quinine every hour by suppository, the same rise in temperature was observed after each. During the night she took her medicine regularly; alcohol and water and beef tea were also adminis-

tered. Sunday morning (the day of her death), I found her to all appearance bloodless, pulse rapid and small, respiration shallow and too frequent, temperature rising to 104° after the wet pack, as before. During Saturday night she had removed the packing from the nostrils, which allowed a slight oozing of blood, and this having been swallowed gave rise to vomiting of clotted blood; this continued, after the bleeding from the nose had been again checked, at intervals of ten to fifteen minutes, which gave me the belief that there was a slight hæmorrhage into the stomach, these clots differing somewhat in form and color from those which I ascribed to the epistaxis. The heart became more rapid—138 per minute—and the patient gradually sank. She died at 6:30, P.M., in great agony, giving two or three shrieks, which were quite loud, considering her weakened condition. Dr. Keating remarked that Dr. William Osler had kindly examined for him a slide of blood and reported as follows: Examination about three hours after withdrawal. *Red corpuscles* present; no special alteration in size or shape. *Colorless corpuscles* greatly increased in number, fifty or sixty in each field of the No. 7 Hartnack. They present remarkable variations in size, many are small, not more than one third the size of the larger forms; they resemble the smaller colorless cells which Virchow has noted to be present in cases of lymphatic leukæmia. Many of the cells have feeble amœboid movements. *Neucleated red corpuscles* not observed. *Schultz's granule masses* (often abundant in leukæmia), scanty.

The relation of the increase in number of the colorless corpuscles above noted to the increase in size of the glands and cytogenetic tissue is indeed hard to solve. The increase in size and hyperplasia of the spleen in leukæmia and anæmia are histologically identical. We must remember that the view that colorless corpuscles are changed into red corpuscles is not fully established, hence, also that it is not proven that the excess of colorless corpuscles is due to failure in the change to red ones. In such cases the prognosis when the disease is detected at its incipiency may be favorable under rigid treatment of fresh air, suitable diet, iron, quinine, and arsenic and salt bathing; but in marked cases that have existed for some time with advanced symptoms, the result is fatal.

DR. WM. GOODELL inquired if there was any distinguishing points between purpura hæmorrhagica and lymphatic leukæmia. If there is a deficiency of red blood corpuscles, why do red patches occur so easily?

DR. KEATING remarked that the subject under discussion was dependent upon certain conditions which physiologists are still debating. In leukæmia we have as a diagnostic feature an involvement of the lymphatic system more or less, a hyperplasia of the tonsils, lymphatic glands of the peritoneum and of the intestines, also of the spleen and bone-marrow, all of them being more or less connected with red cell formation; but the principal diagnostic point is the increase in number of the colorless cells as is noted in Dr. Osler's report just presented. The hæmorrhages in these cases are possibly due to a diapedesis or capillary rupture. In what is known as purpura hæmorrhagica, there is an exudation of blood-cells, or the hæmatin from their destruction, into the rete mucosum and the papillary layer of the cutis; of course capillary ruptures may occur with profuse hæmorrhage. The blood cells (red) are usually diseased; they become crenated, or they cease to form rouleaux, and possibly the plasma may be at fault. The microscope alone will reveal the distinguishing features. Purpura may be considered a symptom accompanying a dyscrasia in which the blood itself is involved, not merely the organs of its production.

(*To be concluded.*)

DOMESTIC CORRESPONDENCE

NEW YORK LETTER.

(FROM OUR OWN CORRESPONDENT.)

New York State Medical Association—Relation of the State to Medical Education—Is Tubercular Consumption ever Inherited—Discussion on Pneumonia; Is it a Local Disease? Is it due to Micro-Organism? Its Fatality; Remedies for It; Blood-letting; Alcohol; Antipyretics.

The meeting of the New York State Medical Association, which was held November 17, 18, 19 and 20, was in every way a most gratifying success, and an enormous amount of scientific work was accomplished at it. During the four days there were no less than eight day sessions and three night sessions, and there was quite a full attendance at all of them.

The address of the President, Dr. John P. Gray, of Utica, was devoted principally to the "Relation of the State to Medical Education," and in it he took very strong ground against the organization of a State Board of Medical Examiners. To take away from corporate schools of medical learning, which were the property of private individuals, and the students of which paid for their own tuition, he said would indeed be an anomaly in political economy. It would only tend to degrade the schools, and would transfer students, who came to New York from all parts of the country and from foreign countries, to Boston, Philadelphia and other cities. The same opposition to a State Board of Examiners was also expressed in the address on State Medicine, delivered by Dr. Alfred L. Carroll, Secretary of the State Board of Health. From his official intercourse with the physicians of the State it was evident, however, that the medical schools turned out many incompetent men, and a considerable portion of the address was devoted to the need of a preliminary education and a more thoroughly enforced scientific training of candidates for the degree of Doctor of Medicine.

Among the papers read on the first day was one on "Tubercular Consumption; Is it ever Inherited?" by Dr. H. D. Didama, of Syracuse. The principal conclusions of Dr. Didama were that tuberculous disease is not inherited, that if a special tendency to the disease is transmitted, the term *liability* expresses the idea better than *tendency*, and that two conditions are always indispensable for the causation of the disease, viz.: abundance of bacilli and an inviting asylum for their development, whether

the susceptibility is inherited or acquired. To the discussion of the subject of shock, short papers were contributed by Drs. Frank H. Hamilton and E. S. F. Arnold.

Undoubtedly the most notable portion of the scientific proceedings during the whole session was the general symposium on the subject of pneumonia, which came off in the evening of this day. The discussion was opened by Dr. Austin Flint, in a brief paper in which he propounded eight questions relative to the nature, pathology, prognosis and treatment of acute lobar pneumonia, and in a clear and judicial manner brought out the most important points bearing upon the various problems connected with the disease. In the course of it he mentioned that he had never seen a case of relapse in this affection, and expressed his conviction that there was no tendency whatever to relapse in it; a point which he considered to argue strongly in favor of pneumonia's being a continued fever. The occurrence of an attack of it, however, did not diminish the susceptibility to a future recurrence of the affection; a point in which it differed from the ordinary essential fevers. A few years since Niemeyer and his school had attempted to show that pneumonia often led directly to pulmonary phthisis; but Louis, more than half a century ago, had satisfactorily demonstrated that it was characterized by no such tendency, and later researches had abundantly confirmed his results.

The first question, Is lobar pneumonia a primary local inflammatory disease, or is it an essential fever, the pulmonary affection being secondary thereto and constituting its anatomical characteristic? was discussed by Drs. Henry D. Didama, of Syracuse, and F. W. Ross, of Elmira, in excellent papers in which they both took the ground of its being an essential fever, and presented in a concise manner the various arguments in favor of this hypothesis. Dr. Didama remarked that three views regarding the nature of this disease had widely prevailed at different times. The first was that, preceding the pneumonic attack, there was a morbid increase of fibrin, with the addition of a certain unknown element, in the blood, the accumulation going on until there was an explosion in the form of a chill, followed by fever; and that the lungs had a special affinity for the blood in this condition. The objection to this view was that the preëxistence of hyperinosis was merely assumed, and not demonstrated. That it was erroneous was shown by the fact that in acute rheumatism hyperinosis constitutes a marked feature; but it does not produce pneumonia. The second was the inflammatory view, which regarded the affection as an ordinary phlegmasia, in which there are predisposing and exciting causes, and in which, when the inflammation has run its course, the disease ends. But, he contended, the ordinary causes of phlegmasia were not sufficient to excite pneumonia, as was the case in bronchitis; however, it could not be produced experimentally, and it differed from the ordinary inflammations in the marked severity of the chill inaugurating it and the high grade of fever attending the attack, which, in the early stages, at all events, could not be accounted for by the local trouble present.

The third view was that it was an essential fever. In connection with this he remarked that the belief seemed to be gaining ground that all communicable diseases are of germ origin. If, however, pneumonia be a constitutional affection, why are the lungs always implicated, and why, as is most frequently the case, but one lung, and only one lobe of that? In reply to this question, he said that many pathologists considered that each essential fever has its special local manifestation, well-marked instances of which could be found in the case of typhoid fever, epidemic cerebro-spinal meningitis, glanders, and other infectious diseases. In most of the general affections which had a local predilection, however, a number of organs were likely to be involved in the diseased process. This was true also of pneumonia, in which the kidneys, liver, spleen and heart were not infrequently implicated. Among the objections which had been raised to this disease being one of the essential fevers was, that in the latter there was a diminution in the quantity of fibrin in the blood, while in pneumonia there was an enormous increase of fibrin. But Andral had shown that the same thing was true of variola in the early stage. Again, it was objected that pneumonia predisposes its victim to a recurrence (not a relapse); but the same thing was true of cholera, erysipelas, diphtheria, and other similar affections. Having referred to the fact that the investigating committee of the British Medical Association had reported that cases of pneumonia have been observed in which it was impossible to detect any breath in the lung, he said, in conclusion, that the labors of Jürgensen, Eberth, Leyden, and others, including our own Flint, had so far established the true nature of this disease that it could no longer be doubted that it was really an essential fever.

Dr. Ross said that at the present time he thought the majority of practitioners would pronounce pneumonia to be a local disease of the lung. For himself, however, the more he saw of it the more and more convinced he became that it was undoubtedly constitutional. Thus, it selected by preference certain ages and conditions, and there was no ratio, as a rule, between the severity of the attack and the amount of pulmonary tissue involved, except in those cases where a very large amount was implicated, where the damage to so extensive a portion of the lungs might by mechanical means produce disastrous results. Ordinarily, the fever and constitutional disturbance were the elements to be taken into consideration, and, when the temperature fell, the whole aspect of the disease was changed. The patient was then on the high road to recovery, notwithstanding the fact that a physical examination showed that the local trouble was as serious as ever, or even more aggravated than before. The case might be different in pneumonias which followed an injury or occurred in connection with some long-continued pathological condition; but Dr. Frederick Roberts, who did not himself believe that the disease was an essential fever, expressly states that such attacks are not those of acute croupous pneumonia. Dr. Ross went on to say that whenever in the latter affection we find the fever not too high and the pulse moderate, we can

look for a good result, notwithstanding the condition of the lungs. Again, the kind of treatment which is most efficacious is that required for a constitutional rather than a local disorder. The common selection of the right lung also strongly points to a constitutional origin, just as the intestinal lesions in typhoid fever do. In the same way it is noticed that the eruption of variola constantly appears first upon the face, and that it shows a special preference for the molar portion of the latter. Diphtheria and scarlet fever, as well as other constitutional affections, also have their peculiar local manifestations. In conclusion, he remarked that a disease which had the essential features of a continued fever, which could not be artificially produced, and in the treatment of which the condition of the heart was more of a guide than the lungs, ought rightly to be classed among the continued fevers.

Dr. E. G. Janeway spoke on the vexed question "What facts and rational grounds, with our present knowledge, can be cited in support of the doctrine that acute local pneumonia depends on the presence of a specific micro-organism?" Having stated that he had been prevented by a severe attack of the disease in question from making a number of personal investigations which he had contemplated in connection with the present discussion, he proceeded to refer to the labors of Klebs, Eberth, Leyman, Friedlander and others, in establishing the identity and etiological efficiency of the pneumo-coccus. While expressing his conviction that this microbe was really the causative agent in pneumonia, he thought it well to call attention to the fact that Dr. George M. Sternberg, U. S. A., had recently claimed that the pneumonia-coccus of Friedlander is identical specifically with a micro-coccus previously described by himself, which is commonly found in normal human saliva. Dr. Sternberg, however, had not, it seemed, made any parallel cultures of the two microbes. In conclusion, Dr. Janeway stated that three days before the meeting he had injected three rabbits with an old pneumonia culture, and that one of them died. The examination showed that there was inflammation of both pleuræ, and that the lung on the side of the body which had been injected was inflamed and solidified, while the other lung was collapsed and exhibited petechial points through its tissues. There was, therefore, undoubtedly septicæmia with pneumonia. (The specimen was exhibited.) The other rabbits are still alive, but one of them was ill and likely to die. A strong case, he said, could be made out in favor of the pneumo-coccus as the cause of pneumonia. The question was, Is the pneumonia obtained by experimentation with this a fair representation of pneumonia as it occurs in the human subject? The nature of the proof demanded for the pneumo-coccus is the same as for microbes in the other affections, viz: 1. The disease-exciting germ must be proved constantly present during the presence of the disease, or at least of its earlier stages. 2. It must admit of cultivation out of the body under such circumstances as to assure us of its purity. 3. Re-introduced into the system, it must excite in susceptible individuals the same disease phenomena. 4. In the individual so made ill by the introduction of the cultures we must again find the same germs as the cause.

The third question, "What conditions incident to acute lobar pneumonia tend to render the disease fatal?" was discussed by Drs. W. H. Robb, of Amsterdam and H. M. Biggs, of New York. The former said that statistics showed that the average mortality was one in five, and that the disease was especially fatal in the very old and the very young. It was more fatal in negroes than whites, and in the female than in the male, the mortality figures in the two sexes bearing the relation of three to two in Vienna. Pregnancy also increased the danger from the disease, and it was more fatal when it attached to the upper lobe of the lung than the lower. Recovery was very doubtful when the whole of one lung was involved, and in double pneumonia recovery was the exception. In chronic drunkards the danger was nearly doubled, and it was also great in cases with such complications as endocarditis, pericarditis, and emphyzema. It was also noticeable that the disease was more fatal in some years than in others. When the pulse reached 130 or 140 and remained thus rapid for any length of time, the case generally ended fatally, and an intermittent pulse indicated great danger. A temperature of 105° or over was a bad sign, and muttering delirium and subsultus tendinum were usually of fatal import. Dr. Biggs stated that in acute pneumonia the tendency was to recovery, and when this did not occur the fatal issue to be referred to other conditions complicating it. Among them were the existence, (1) of some unfavorable condition as regards habits, constitution, age, etc.; (2) of some other acute or chronic disease; (3) of certain complications developed in the course of the disease; (4) of the involvement of more than one lung; and (5) of intense activity of the morbific principle. Inquiring whether it was possible to trace any circumstance which was common to all the different conditions rendering the prognosis bad, he said that he believed it was, and this was heart-failure. He considered pneumonia as an essential fever which has its local manifestation in the lung, which is infectious in the same sense that typhoid fever is, and which is due to a specific cause.

In discussing the fourth question, "Are there known remedies or therapeutic measures capable of arresting the disease, or of exerting a curative influence by either shortening its duration or conducing in any way to a favorable termination?" Dr. T. F. Rochester, of Buffalo, advocated with much earnestness the special efficacy of carbonate of ammonia, the advantages of which he had first learned when serving in the hospital in, 1848, under Alonzo Clark and John T. Metcalfe. It was important that the solution should be made from the freshly prepared crystals, and that it should not be kept in the form of powder. He advised that it should be used in doses of from five to ten grains every two hours, and that it should as a rule be given in milk. In the old and debilitated as much as ten grains every hour might be required. With the carbonate of ammonia he usually employed about five grains of Dover's powder, or an eighth of

a grain of morphine, every four hours. When alcohol was indicated, it was sometimes wonderful to see what it could accomplish, and a highly nutritious diet was always of service. He did not believe in the use of the arterial sedatives, but digitalis and convallaria were sometimes of service in stimulating the heart's action. Quinine, kairin and antipyrin were also useful, but he thought that the employment of the latter might be carried too far.

The fifth question was, "Is blood-letting ever indicated in this disease, and if so, what are the circumstances indicating and contra-indicating this measure of treatment?" and it was discussed by Dr. Simon T. Clark, of Lockport. Possessed of a strikingly handsome presence, a clear and well-modulated voice, a graphic style of writing and no little elocutionary power in delivery, Dr. Clark undoubtedly made the oratorical hit of the evening, being listened to with profound attention and frequently greeted with hearty applause. Believing that the disease was an inflammatory affection, he said there was ground for supposing that, like any other inflammation, it might originate from a variety of causes. In the course of his remarks he stated that the experience of physicians in large city hospitals should not be accepted with too much confidence by country practitioners as always applying to their own patients, and related two cases in which he was confident that he had saved life by the use of the lancet. One case, in which he drew sixteen ounces of blood, was that of a robust young man whose system was overwhelmed with complete stasis at the commencement of an attack of pneumonia; and the other, in which he drew somewhat less than eight ounces, was that of an Irish woman 60 years of age, accustomed to hard manual labor, who was suffering from passive pulmonary œdema on the fifteenth day of the disease; stimulants producing no effect whatever, and death apparently being imminent. These two conditions, he believed, were indications for the infrequent but judicious use of the lancet, and the result was the test of its efficacy.

Drs. John Shrady, of New York, and E. D. Ferguson, of Troy, discussed the sixth question, "Is alcohol useful in the treatment of acute lobar pneumonia, and if so, what are the indications for its use, and how is its use to be regulated as regards the quantity given, etc.?" Dr. Shrady said that the faith in the efficacy of alcoholic stimulus was too deeply rooted to be disputed; but he believed that it was overrated. It was supposed that the more rapid the pulse the greater the need of alcohol, and that it was especially indicated in intermittent pulse. Still it was only a forlorn hope. He did not believe that it had the effect of lowering the temperature. In cases where it seemed to be required from four to eight ounces of spirits a day would usually prove sufficient, and he thought it was best to give it in the form of milk punch. Dr. Ferguson said that he was not aware that a comparison of two sets of parallel cases, in one of which alcohol was used, and in the other not, had ever been made. Having referred to a number of authorities, he said that it could not be doubted that there exists a general opinion that alcohol is of service in quite a large per centage of cases of pneumonia. In some cases the increased tolerance of alcohol was quite remarkable. In others, its use was attended with extreme danger. There was no reason to doubt that alcohol was decomposed within the human organism, and being thus a generator of force, must therefore be regarded as a food. It was of special service in cases where the time required before ordinary food could be digested would lead to a fatal result; the alcohol furnishing energy without any demand upon the tonus of the body. It temporarily increased the strength of the heart muscle, and he believed that it also acted as an antiseptic. He had not been able to find contra-indications to the use of alcohol in moderate doses in in any cases of pneumonia, and he was at a loss to understand the feeling against it. If the censure was upon the immoderate use of this agent, as well might we inveigh against the immoderate use of arsenic or opium. As a rule alcohol should be given at frequent intervals, as the effect on the heart was evanescent; and he thought that from one half to one ounce of whisky, repeated every one or two hours, was required.

The discussion was resumed on the second day, when Dr. C. G. Stockton, of Erie county, read a paper on the seventh question: "To what extent is it safe and useful to employ antipyretic measures of treatment in cases of acute lobar pneumonia, inclusive of the cold bath, spraying the body, or the wet sheet?" With the possible exception of cold sponging, he condemned the use of external antipyretic measures. The selection of an internal antipyretic, he thought, should be made according to the indications of the case. As to antipyrin, he believed that it should never be given in doses larger than 20 grains, nor at intervals of less than eight or ten hours.

The eighth and last question: "Do relapses of acute lobar pneumonia ever occur during or shortly after convalescence, and does this disease involve any special liability to other diseases, or sequelæ?" was discussed by Dr. J. G. Orton, of Binghamton, who stated that he had never seen an instance of such relapse, and did not believe that they ever occur, the so-called relapses being merely due to complications or the involvement of a new portion of the lung. Bronchitis, pleurisy, acute nephritis and pericarditis were likely to result indirectly from pneumonia, and one attack of the latter disease predisposed to subsequent attacks. Some general debate then brought this memorable discussion to a close. P. B. P.

NECROLOGY.

ALONZO T. KEYT, M.D.

Dr. A. T. Keyt, of Cincinnati, died at his home in that city on November 9, of cardiac paralysis. He was born in Higginsport, Ohio, January 10, 1827, and was therefore in the 59th year of his age. While he was a boy his parents moved to Moscow, Ohio. After graduating at Felicity, he went to Cincinnati as a medical student, and attended the Ohio Medical College, and, upon attaining the highest honors at that

institution, was appointed resident physician at the Cincinnati Hospital with Dr. M. T. Carey. After his marriage, in 1849, he settled in Moscow, but soon returned to Cincinnati, where he built up a most enviable practice and reputation.

Dr. Keyt was devoted to his profession, and was an eminent writer on medical subjects, having been a valuable contributor to the *Boston Medical and Surgical Journal*, *New York Medical Journal*, *The Cincinnati Lancet and Clinic*, THE JOURNAL OF THE AMERICAN MEDICAL ASSOCIATION, and the *London Lancet*. He was the inventor of the compound sphygmograph, which bears his name (Keyt's Cardiosphygmograph)—an instrument for recording at once the beating of the heart and the pulse, which received in its infancy the hearty endorsement of Marey and other leading scientists engaged in the development of graphic methods of investigation. He was a patient, laborous and conscientious worker. He never submitted anything to the medical press until after repeated trials he was fully convinced of the correctness of his conclusions. Through his writings Dr. Keyt was better known in Europe than in his own country; his contributions to scientific journals on the sphygmograph, and its use, were quoted as authority. One of his last contributions to medical literature, if not his last, was a paper on "Cardiography," read at the meeting of the American Medical Association, last spring, and published in THE JOURNAL of August 8, 1885.

Dr. Keyt was a hard worker, and had been for the thirty-five years he had practiced medicine.

The post-mortem examination, as made by Drs. Dunn and Isham, showed death to have been due to overwork.

MISCELLANEOUS.

NATIONAL CONFERENCE OF STATE BOARDS OF HEALTH—In accordance with a resolution adopted at its organization, The National Conference of State Boards of Health will hold its regular annual meeting at the Willard Hotel, Washington, D. C., December 8, 1885. For the convenience of the many sanitarians who may desire to attend both, the annual meetings of the Conference are held at the same time and place as those of the American Public Health Association, but its sessions are so arranged as not to conflict with the work of the latter organization. With this idea in view, and at the suggestion of the President, the preliminary session of the Conference will be called to order at 9 A. M., December 8.

Members proposing to present papers to this meeting are requested to send early notice to the Secretary,

J. N. MCCORMACK, M.D.

Bowling Green, Ky.

YELLOW FEVER INOCULATION.—We learn that Dr. Joseph Holt, of New Orleans, has received a letter from Dr. J. McF. Gaston, of Atlanta, Ga., inclosing a communication from Dr. Domingo Freire, of Rio de Janeiro, giving interesting information about inoculation for yellow fever. Dr. Freire stated that he had this year performed nearly 5,000 inoculations, the result of which proved an absolute success, for none of those who were inoculated died; whereas over 400 persons who were not inoculated have already succumbed to the disease. The inoculated persons dwelt in the same infected quarter, and a great number inhabited the houses wherein deaths had occurred.

OFFICIAL LIST OF CHANGES IN THE STATIONS AND DUTIES OF OFFICERS SERVING IN THE MEDICAL DEPARTMENT, U. S. ARMY, FROM NOVEMBER 14, 1885, TO NOVEMBER 20, 1885.

Capt. F. W. Elbrey, Asst. Surgeon, sick leave of absence further extended six months on surgeon's certificate of disability. (S. O. 263, A. G. O., Nov. 14, 1885.)

Capt. Norton Strong, Asst. Surgeon, relieved from duty at Ft. Union, N. M., and ordered for duty as attending surgeon, Hdq'rs Dist. of New Mexico, and post surgeon Ft. Marcy, N. M. (S. O. 171, Dept. Mo., Nov. 16, 1885.)

First Lieut. C. B. Ewing, Asst. Surgeon, now at Ft. Leavenworth, Kansas, ordered to proceed to Ft. Reno, Ind. Ter., and report to commanding officer for temporary duty in the field. (S. O. 170, Dept. Mo., Nov. 13, 1885.)

OFFICIAL LIST OF CHANGES IN THE MEDICAL CORPS OF THE U. S. NAVY, DURING THE WEEK ENDING NOVEMBER 21, 1885.

M. C. Drennan, Surgeon, ordered to training ship "New Hampshire."

H. Aulick, Surgeon, detached from training ship "New Hampshire," and wait orders.

H. H. Fitts, Asst. Surgeon, ordered to appear before Examining Board preliminary to promotion.

C. W. Rush, P. A. Surgeon, granted sick leave for three months from Nov. 20, 1885.

CORRIGENDA.

In the JOURNAL of October 3, p. 387, second column, in giving the conclusions of the paper on "Natural and Ideal Cholecystotomy," by A. C. BERNAYS, M.D., of St. Louis, for "The paper concludes with the following propositions," *et seq*, read:

"Having now arrived at the end of the work, I may be permitted to offer my own conclusions, and I respectfully submit them as follows:

"I. The causes which indicate an operative interference with the system of gall vessels are: *a*, jaundice; *b*, paroxysmal pain or a tumor in the right hypochondriac region; *c*, suppuration; *d*, peritonitis; these conditions to be either severally or singly recognizable, the presumable origin being biliary calculi; *e*, malignant disease.

"II. Explorative laparotomy must be preferred to acupuncture or aspiration as a diagnostic measure.

"III. The incision in the linea alba is preferable when there is much doubt regarding the seat of obstruction, because the large ducts can be reached much better from this incision than from the incision parallel to the free border of the ribs.

"IV. The escape of bile through an abdominal fistula is not injurious to the process of normal digestion. The bile is an excretion, and probably of no more use in the intestinal canal than the urine in the bladder.

"V. Jaundice, when caused by an obstruction of the common duct, is no contraindication to natural cholecystotomy. We may often save life by its early performance.

"VI. Cholecystotomy, natural and ideal, and cholecystectomy are the three operations at our service; cholecystenterostomy may be useful, but it has not yet earned a place among approved surgical procedures.

"VII. Ideal cholecystotomy is indicated when the bladder is normal in structure and when the gall-ducts have been cleared of obstructing calculi.

"VIII. Natural cholycystotomy is indicated when the bladder is ulcerated or suppurating, or when there are permanent obstructions beyond reach at the time of operation.

"IX. Cholecystectomy should be limited to cases of otherwise incurable or malignant disease of the gall-bladder."

In my paper upon "The Rational Treatment of Pulmonary Diseases by the Pneumatic Cabinet," published in the JOURNAL of Nov. 14, 1885, I mention a case reported by Dr. V. Y. Bowditch before the Suffolk District Medical Society on Nov 11, in which bacilli had been seen to disappear from the sputa, and I *inferred* from conversations I had had with Dr. B. that the treatment employed had been the Pneumatic Cabinet. As soon as I learned my mistake—that the patient had never been under this treatment—I telegraphed to have the paragraph omitted, but I was too late—the unfortunate sentence was already in print. In justice, however, both to Dr. Bowditch and myself, I wish to make this public correction of my error—an error, as Dr. B. kindly writes me, "such as anyone might easily have made."

W. EVERETT SMITH, M.D.

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